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THE

EDINBURGH NEW
PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

CONDUCTED BY

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OCTOBER 1835...APRIL 1836.

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CONTENTS.

ART. I. Biographical Memoir of M. de Lamarck. By the Baron CUVIER, - - - - - - Page 1

II. On the Hindu Astronomical Tables. Communicated by the Author, - - - - - - - 22

III. On a Species of Beroe found on the north-east coast of Ireland. By ROBERT PATTERSON, Esq. Treasurer to the Belfast Museum. Communicated by the Author, - - - - - - - 26

IV. Reply to Dr John Davy's Remarks on Certain Statements by Mr Faraday, contained in his "Researches on Electricity," in the Edinburgh Philosophical Journal for October 1835. By MICHAEL FARADAY, D. C. L., F. R. S., &c. &c. - - 37

V. An Account of Professor Ehrenberg's more recent Researches on the Infusoria, - - - 42

- Section I. On the existence of a Pharynx and of teeth in the Polygastric Infusoria, - - ib.
- II. Concerning a System of Internal Organs, simple, double, or multiple, very irritable, which may be discovered in the Polygastrica, and which, perhaps, are the Male Sexual Organs, - - - - - 45
- III. Concerning a violet or very deep blue coloured liquid, which is found in the Intestines of the Polygastrica, and concerning the particular organs which secrete it, 51
- IV. Upon Internal Organs like Branchiæ, which have been discovered in the Rotatoria, 54
- V. On the Nervous System of the Infusoria, 60

ART. VI. Meteorological Observations made at Fort Vancouver, from June 7. 1833, to May 31. 1834. By Dr M. GAIRDNER. Communicated by the Author, - - - - - - - - 67

VII. On the Chalk and Flint of Yorkshire, as compared with the Chalk and Flint of the Southern Counties of England. By JAMES MITCHELL, LL. D. F. G. S. Communicated by the Author, - 68

VIII. On the Infra-orbital Cavities in Deers and Antelopes, called Larmiers by the older French Naturalists. By ARTHUR JACOB, M. D., Professor of Anatomy in the Royal College of Surgeons in Ireland. Communicated by the Author, - 74

IX. Remarks on the Difficulty of distinguishing certain Genera of Testaceous Mollusca by their Shells alone, and on the Anomalies in regard to Habitation observed in certain Species. By JOHN EDWARD GRAY, Esq. F. R. S. &c. - - - 79

- I. On Shells apparently similar, but belonging, on a comparison of their Animals, to very different Genera, - - - 81
- II. Of Species belonging to the same natural Genus, inhabiting essentially different situations, - - - - - - 86

X. On some Circumstances connected with the Original Suggestion of the Modern Arctic Expeditions. Communicated by the Rev. W. SCORESBY, B. D., 93

XI. On the Causes of Obstruction in Water-pipes and Syphons from Disengaged Air; and on the Construction of a Hydraulic Air-extractor for Removing them. By J. STEWART HEPBURN, Esq. of Colquhalzie, Mem. Soc. Arts, - - - 100

- Report by Mr Dunn, - - - - 112
- Report by Mr George Buchanan, - - ib.
- Report by Mr Clerk Maxwell, - - 113
- Copy Letter from James Hunter, Esq. of Thurston, to the Chairman of the Water Company, Edinburgh, dated 20th January 1821, referred to in the previous Report, - -

Art. XII. On a Curious Phenomenon observed in the Island of Cephalonia, and on the Proximate Cause of Earthquakes in the Ionian Islands. By JOHN DAVY, M. D., F. R. S., Surgeon to the Forces. Communicated by the Author, - - - - 116

XIII. Account of the Great Suspension Bridge at Fribourg, 123

XIV. On the Volcanic Formations of the Environs of Naples. By M. DUFRENOY, - - - 126

I. Different Epochs of the Volcanic Phenomena, ib.

II. Pumice-Tuff, - - - - - 127

III. Trachytes of the Phlegræan Fields, - 128

IV. On Vesuvius, - - - - ib.

V. Oscillatory Movements of the Surface of the Campania, - - - - - 129

VI. Destruction of Pompeii and Herculaneum, ib.

XV. Description and Drawing of a New Pivot-Castor for Furniture, possessing the advantage of retaining the Oil for an indefinite length of time. By JOHN ROBISON, Esq. F. R. S. E., Vice-Pres. Soc. Arts, 130

XVI. Short Account of the Rev. John Flamsteed, the first Astronomer-Royal. By FRANCIS BAILY, Esq. Vice-Pres. of the Astrom. Soc. &c. - 131

XVII. On the Quadrupeds and Birds inhabiting the County of Sutherland, observed there during an Excursion in the Summer of 1834. By J. P. SELBY, F. R. S. E., F. L. S. &c. Communicated by the Author, - - - - - - - - 156

XVIII. Notice concerning the Life and Writings of Professor Brinkley, Bishop of Cloyne. By M. ARAGO, 161

Chronological Catalogue of the Memoirs published by Brinkley, - - - - - 163

XIX. Eruptions of the Volcano of the Cosiguina. By Colonel JUAN GALINDO, - - - - - 165

XX. On the Nature of the Light of Comets. By M. ARAGO, - - - - - - - - 170

XXI. Report respecting the Statistical Researches of Dr Civiale on Calculous Affections made to the Academie des Sciences. By Messrs POISON, DULONG, LARREY and DOUBLE. October 5. 1835, 173

XXII. On Foot-marks of Animals in Rocks, - - 179

XXIII. Memoir entitled, "Researches on the Structure and Origin of Mount Etna." By M. L. ELIE DE BEAUMONT, - - - - - - 185

XXIV. Phases of the Annular Eclipse of the Sun, which will happen Sunday May 15. 1836, calculated for the Observatory of Edinburgh. By Mr ROBERT TREAT PAINE, of Boston, United States. Communicated by the Author, - - - - 186

XXV. Description of several New or Rare Plants which have lately flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr GRAHAM, Professor of Botany, - - 189

XXVI. Proceedings of the Wernerian Natural History Society, - - - - - - - 197

XXVII. Proceedings of the Society for the Encouragement of the Useful Arts in Scotland, - - - - 201

XXVIII. List of Patents granted in Scotland from 19th September to 9th December 1835, - - - - 202

XXIX. Scientific Intelligence, - - - - - - 204

- 1. Sir Charles Bell, - - - - - - ib.
- 2. Professor David Don, - - - - - - ib.
- 3. Aurora Borealis—Edinburgh, Nov. 18. 1835, - 205
- 4. Composition of the Atmosphere, - - - ib.
- 5. Climate of Fort Vancouver, - - - - ib.
- 6. Geology, - - - - - - - - 206
- 7. Age of the Molasse of Switzerland, - - - 207
- 8. Effect of Cold on the Fur of the Hudson's Bay Lemming, - - - - - - - - ib.
- 9. Effect of Intense Cold on Caterpillars, - - ib.
- 10. Polar Bear, - - - - - - - 208
- 11. The Black Whale, - - - - - - ib.
- 12. Passenger Pigeon, - - - - - - 209
- 13. Spontaneous Plants, - - - - - - ib.

XXX. New Publications, - - - - - - - ib.

CONTENTS.

Art. I. Biographical Memoir of Dr Thomas Young. By M. Arago, ... Page 213

II. On the Powers and Use of Kater's Altitude and Azimuth Circle. By Mr W. Galbraith, . 241

III. Remarks on the Arrangement of the Natural Botanical Families. By Sir Edward Ffrench Bromhead, Bart. F. R. S. Lond. and Edin. Communicated by the Author, . . . 245

Sketch of an Arrangement of the Botanical Families in Natural Groups, Alliances, and Races, . 251

IV. Abstract of the Memoirs of John Napier of Merchiston. By M. Biot. With Notes by the Translator, ... 255

V. On the Quadrupeds and Birds inhabiting the County of Sutherland, observed there during an Excursion in the Summer of 1834. By P. J. Selby, Esq., F. R. S. E., F. L. S., &c. &c. . . . 286

VI. Memoir on the Star-Fish of the genus Comatula, demonstrative of the Pentacrinus europæus being the Young of our Indigenous Species. By John V. Thompson, F. L. S., Dep. Inspector-General of Hospitals. Communicated by Sir James M'Grigor, F. R. S. With a Plate, . . . 295

VII. On the Chemical Constitution of Gadolinite. By A. Connell, Esq. F. R. S. E., &c. Communicated by the Author, ... 300

VIII. Description of a New Detached Pendulum Escapement; invented by Alexander Witherspoon, Watchmaker, Tranent, ... 303

IX. On the Occurrence of the Megalichthys in a Bed of Cannel Coal in the West of Fifeshire, with Observations on the supposed Lacustrine Limestone at Burdiehouse. By LEONARD HORNER, Esq. F.R.SS. L. & E., Fellow of the Geological Society. Communicated by the Author, . . . 309

X. Remarks on the Dublin and Kingstown Railway, intended as a Supplement to a former Paper on the Liverpool and Manchester Railway, in the 18th Volume of this Journal, 1835. By DAVID STEVENSON, Esq., Civil-Engineer, Edinburgh. With a Plate, ... 320

XI. Description of a Single Reflecting Microscope. By ALEX. GUTHRIE, Esq. Communicated through J. Robison, Esq. Sec. R. S. E., . . 326

XII. Notice of some minute Calculi found in the Urinary Bladder of an Ox. By JOHN DAVY, M. D., F. R. S., Assistant-Inspector of Army Hospitals, &c. Communicated by the Author, . . . 327

XIII. On the Cause of the Temperature of Hot and Thermal Springs; and on the bearings of this subject, as connected with the general question regarding the Internal Temperature of the Earth. By Professor GUSTAV BISCHOF of Bonn. Communicated by the Author, ... 329

PART FIRST.—What Thermometrical Circumstances on the Surface of the Earth lead us to assume that an Increase of Temperature towards the Centre of the Earth must take place? 329

CHAP. I.—On the Circumstances under which Warm Springs are found on the Surface of the Earth, and on their frequency of occurrence, . 330

CHAP. II.—Can the elevated temperature of Acidulous Springs be a consequence of the absorption of Carbonic Acid Gas? 336

CHAP. III.—Can the heat of Thermal Springs be the result of chemical processes or of local circumstances? and can local circumstances cause any modifications in the temperature of Thermal Springs? 343

CHAP. IV.—Can Springs convey Heat from the Interior of the Earth to the Surface? . 355

CHAP. V.—The Temperature of Springs being a function of that of the Meteoric Waters, and of the Strata of the Earth through which they flow, it is required to determine whether the Variations of the Temperature of the Meteoric Waters also shew themselves in Thermal Springs, . . 359

CHAP. VI.—Can the Mean Temperature of a place be determined from the Temperature of Springs? and is the Mean Temperature of the Soil the same as that of the Air? . . . 362

XIV. Analysis of a Memoir on the Structure and on the Origin of Mount Etna. By M. L. ELIE DE BEAUMONT, Member of the Royal Academy of Sciences of the Institute. Communicated by the Author,

XV. Questions for Solution relating to Meteorology, Hydrography, and the Art of Navigation. By M. ARAGO, ... 393

1. Meteorological Phenomena, ... 394
2. Observations designed to characterize the present state of the Globe in regard to Temperature, . 395
3. Of the Calorific Action of the Solar Rays viewed in their relation to the situation of places on the Globe, 396
4. Experiments to be made on the Radiation of the Sky, 397
5. Examination of an Anomaly which Atmospheric Temperatures, taken at different elevations, present in the night, when the sky is calm and clear, . 399
6. Expeditious Method of determining Mean Temperatures in Equinoctial Countries, . . 400
7. Observations to be made on Thermal Springs, . 401

XVI. Abstract of an Address delivered on presenting the Keith Medal, adjudged by the Council of the Royal Society of Edinburgh to Professor Forbes, for his Experiments on the Polarization of Heat; by Dr HOPE, Vice-President of the Society, . 405

XVII. Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr Graham, Professor of Botany, . 412

XVIII. Proceedings of the Royal Society of Edinburgh, 414

XIX. Proceedings of the Wernerian Natural History Society, ... 418

XX. Proceedings of the Society for the Encouragement of the Useful Arts in Scotland, . . 419

XXI. Scientific Intelligence, . . . 423

1. On the supposed existence of a New Small Planet, 423
2. Climate of Palestine, ... 425
3. Indications of a Change in the relative Levels of the Land and Sea on the West Coast of Scotland, . 425
4. Facts relating to the Soulevement or Rising of Scandinavia at a recent epoch, . . . 425
5. M. de Collegno on the Soulevements of the Hills of Superga, ... 428
6. On the presence of Cobalt and other Metals in the Upper Sandstones of the Tertiary Formations of Paris, 428
7. On the Colours of Flowers, . . . 429

XXII. List of Patents granted in Scotland from December 18. 1835, to March 18. 1836, . . 430

Index, ... 435

Errata in Dr Davy's paper "On a curious Phenomenon in Cephalonia,"
in No. 39, *January* 1836.

Page 116, line 18, *for* phenomena *read* phenomenon
— 117, — 8, *for* Angostoli *read* Argostoli
— 117, — 24, *for* Hence *read* Here
— 119, — 33, *omit* less *before* absorption
— 120, — 16, *for* Alleschimo *read* Allefechimo
— 121, — 3, *for* lightly *read* highly
— 121, — 5, *for* several *read* mural
— 122, — 25, *for* some *read* soon

To Correspondents.—The various Meteorological Tables, &c. communicated for the Journal will appear in our next Number.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Biographical Memoir of M. de Lamarck. By the Baron CUVIER.

AMONG the men devoted to the noble employment of enlightening their fellows, a small number are to be found (and you have just witnessed an illustrious example*), who, gifted at the same time with a lofty imagination and a sound judgment, embracing in their vast conceptions the entire field of the sciences, and seizing with a steady eye whatever afforded the hope of discovery, have laid before the world nothing but certain truths, establishing them by evident demonstrations, and deducing from them no consequences but such as were irresistible, never allowing themselves to be led away by what is conjectural or doubtful; men of unequalled genius, whose immortal writings will shed a light, like so many phari, on the paths of science, as long as the world is governed by the same laws.

Others, with minds not less ardent, nor less adapted to seize new relations, have been less severe in scrutinizing the evidence; with real discoveries with which they have enriched science, they have mingled many fanciful conceptions; and, believing themselves able to outstrip both experience and calculation, they have laboriously constructed vast edifices on imaginary foundations, resembling the enchanted palaces of our old romances, which vanished into air on the destruction of the talisman to which they owed their birth. But the history of these less favoured philosophers is not perhaps the least useful. While the former should be unreservedly held up to our admiration, it is equally important that the latter should form the subject of our study. Nature alone produces genius of the first order; but it is competent to every laborious man to aspire to a rank among those who have done service to science, and that rank will be the more elevated in proportion as he has learned to distinguish by marked examples the objects accessible to his exertions, and the difficulties which may oppose his progress. It is with this view, that, in sketching the life of one of our most celebrated naturalists, we have conceived it to be our duty, while bestowing the commendation they deserve on the great and useful works which science owes to him, likewise to give prominence to such of his productions in which too great indulgence of a lively imagination has led to results of a more questionable kind, and to indicate, as far as we can, the cause, or, if it may be so expressed, the genealogy of his deviations. This is the principle by which we have been guided in all our historical eloges, and, far from thinking that we have been thereby wanting in the respect due to the memory of our associates, we conceive that our homage is rendered purer, just because it is carefully freed from all that was unworthy of them.

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* This Memoir, not yet published, was designed to follow that of Volta, read by M. Arago at the meeting on the 27th of June 1831, for which see the 16th vol. of this Journal. It was read at the meeting of the French Academy of Science, 26th November 1832.

JEAN BAPTISTE PIERRE ANTOINE DE MONET, otherwise named the Chevalier de Lamarck, was born at Bazantin, a village in Picardy between Albert and Bapaume, on the 1st August 1744. He was the eleventh child of Pierre de Monet, superior of the place, of an ancient house of Bearn, but whose patrimony was quite inadequate to the support of such a numerous offspring. The church, at that period, offered a ready resource, and sometimes a large fortune, to the cadets of noble families, and M. de Monet made an early choice of that destination for his son. As a preliminary step, he was sent to study under the Jesuits at Amiens; but the boy's inclination by no means responded to his father's wishes. All that surrounded him spoke another language: for ages his relations had carried arms; his eldest brother fell in the

t the siege of Bergen-op-Zoom; two others were still in the service; and the moment when France was so actively engaged in the dismal struggle begun in 1756, was not one fitted to discourage a young man of spirit from following such examples. His father, however, opposed this desire; but the good old man having died in 1760, no consideration could prevail on the youthful abbé to adhere to his profession. He set out for the army of Germany on a wretched horse, followed by a poor youth from his village, provided with no other passport than a letter from one of his neighbours, a Madame Lameth, directed to M. de Lastic, colonel of the regiment of Beaujolois. It is easy to conceive the annoyance of this officer at finding himself embarrassed with a boy, whose puny appearance caused him to be thought younger than he really was; he ordered him, however, to his quarters, and continued his duties. The moment in fact was a critical one. It was the 14th of July 1761, when the Marshal de Broglie, having united his army to that of the Prince of Soubise, designed next day to attack the allied army, commanded by Prince Ferdinand of Brunswick. At the dawn of day, M. de Lastic inspected his troops, and the first person whom he saw was the young stranger, who, without saying a word, had placed himself in the first rank of a company of grenadiers, and nothing could induce him to quit his station.

It is well known that this battle, which bears the name of the little village of Fissingshausen, between Ham and Lippstadt, was lost by the French, and that their two generals, mutually accusing each other of the defeat, immediately separated, and undertook no important measure during the rest of the campaign. In the vicissitudes of the contest, the company to which M. de Lamarck had attached himself was placed in a situation which exposed it to the whole fire of the enemy's artillery: in the confusion of the retreat it was forgotten and left there. Already all the officers were killed, and only fourteen men remaining, when the oldest grenadier perceiving that there were no longer any French within sight, proposed to the young volunteer so speedily become commander, to withdraw this little troop. "This post has been assigned us," replied the boy; "we must not quit it unless we are relieved;" and, in fact, he caused them to remain till the colonel, seeing that this company was wanting, sent an order, which could now reach its destination with the utmost difficulty. This instance of firmness having been reported to the Marechal, he instantly gave M. de Lamarck a commission, although his instructions required him to be very sparing in promotions of that nature. Soon after, M. de Lamarck was nominated to a lieutenancy; but such a successful commencement of his military career was not attended with the consequences that might have been expected, for a most unfortunate accident removed him altogether from the service, and entirely altered his destination. When his regiment was in garrison at Toulon and Monaco, one of his companions, in play, lifted him by the head, and occasioned a serious derangement in the glands of the neck. He was obliged to repair to Paris for more skilful treatment than these places afforded, but the efforts of the most celebrated surgeons had no effect; and the danger was become very imminent, when our late associate M. Tenon, with his usual penetration, perceived the nature of the disorder, and effected a cure by a complicated operation, the marks of which always continued visible. This confined him for a year, and during that time, the extreme slenderness of his resources kept him in solitude, which afforded ample leisure for reflection.

The profession of arms had not caused him to lose sight of the notions of physics he had acquired at college.

During his stay at Monaco, the singular vegetation of that rocky country had attracted his attention, and the Traité des Plantes Usuelles of Chomel having accidentally fallen into his hands, inspired him with some taste for botany. From his lodging in Paris, which, by his own account, was much higher than accorded with his wishes, the clouds formed almost his only spectacle, and their varied aspects suggested his earliest ideas of meteorology—a subject which could not fail to interest a mind always distinguished for activity and originality. He now began to perceive, as Voltaire has said of Condorcet, that lasting discoveries might confer on him a different kind of celebrity from a company of infantry.

The resolution which he formed in consequence, was not less firmly adhered to than the first. Reduced to an alimentary pension of 400 francs, he determined to become a doctor, and until he could obtain time for the requisite studies, he laboured assiduously for his daily bread in the office of a banker. His reflections, however, and the contemplations which he delighted to indulge, afforded him consolation, and when he found an opportunity of communicating his ideas to some friend, of discussing them, and defending them against objections, the real world was nothing to him; his warmth made him forget all his difficulties. It is in this way that many men have passed their youth, who have become the lights of the world. Too often is genius born to poverty; but there is in it a principle of resistance against misfortune, and adversity is perhaps the surest test by which it can be tried. Never ought the most unfortunate of young men to forget, that Linnæus was preparing himself to become the reformer of Natural History, at the time when he was patching up for his own use the cast-off shoes of his companions.

At last, after having occupied ten years in preparing himself, M. de Lamarck made himself suddenly known, both to the world and men of science, by a work on a new plan, and executed in a manner full of interest.

He had been for a long time accustomed, when collecting plants, or visiting the Jardin du Roi, to engage in warm discussions with his fellow students on the imperfections of all the systems of arrangement then in vogue, and to maintain how easy it would be to form one which would lead with greater ease and certainty to the determination of plants. His friends in some measure defied him to the task; he immediately set about proving his assertion, and after six months of unremitting labour, finished his "Flore Française."* This work has no pretensions to add to the number of species previously known as indigenous to France, nor even to give a more complete history of them. It is merely a guide which, by setting out from the most general forms, dividing and subdividing always by two, and only allowing the choice between two opposite characters, conducts the reader, however little he may understand descriptive language, as it were by the hand, with certainty, and even amusement, to the determination of the plant of which he desires the name. This kind of dichotomy or continual bifurcation, is im-plied in all methods of arrangement, and even forms the necessary foundation of them, but modern authors, for the sake of brevity, have attempted to present many ramifications together. M. de Lamarck, in imitation of some of the old botanists, developed and expressed them all, representing them by *accolades*, in such a manner that the most uninstructed reader, without any initiatory labour, by taking him for a guide, may suppose himself to be a botanist. His book appeared at a time when botany had become a popular science, the example of J. J. Rousseau, and the enthusiasm which he inspired, having even caused it to be studied by ladies and people of fashion; the success of the work was therefore rapid. M. de Buffon, not perhaps unwilling to shew by this example how easily systems, on which he set so little value, could be framed, and at the same time their indifferent consequence, used his interest to have the Flore Françaiseprinted by the royal press. A place having become open in the botanical department of the Academy of Sciences, and M. de Lamarck being presented in the second rank, the Minister caused it to be given to him by the King in 1775, in preference (a thing almost unexampled,) to M. Descemet, who was presented first, and who has never been able, during a long life, to recover the station of which the preference deprived him. In short, the poor officer, so little regarded since the commencement of the peace, all of a sudden attained to the good fortune, always of rare occurrence, and particularly so then, of being at the same time an object of favour with the court and with the public. The partiality of M. de Buffon obtained for him another advantage. When his son was about to set out on his travels, after finishing his studies, M. de Buffon proposed to M. de Lamarck to accompany him; and not wishing that the latter should appear merely in the character of a preceptor, he procured for him the commission of botanist to the King, for the purpose of visiting foreign gardens and cabinets, and opening a correspondence between them and similar establishments in Paris. In consequence of this he travelled in company with the younger Buffon during part of the years 1781 and 1782, through Holland, Germany, and Hungary; visited Gleditsch at Berlin, Jacquin at Vienna, Murray at Goettingen, and obtained an idea of the magnificent establishments devoted to botany in many foreign countries, to which our own do not yet approach, notwithstanding all that has been done for them for the last thirty years.

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* Flore Française, ou description succincte des toutes les Plantes qui croissent naturellement en France. 3 vols. in 8vo, Paris, 1778.

Shortly after his return, he commenced more important works than his Flora, although less widely known, and which have procured for him a more eminent rank among botanists,—I mean his *Dictionary of Botany,*[*] and his *Illustrations of Genera,*[†] both of which form a part of the Encyclopedie Methodique.

These generic illustrations are perhaps better calculated than any other work for conveying a speedy and accurate knowledge of this beautiful science. The precision of the descriptions and definitions of Linnæus is accompanied, as in the institutions of Tournefort, with figures fitted to embody their abstractions, and to present them to the eye as well as to the mind. Nor will the student have the means of becoming acquainted with the fruits and flowers only; the whole appearance and habits of one or two of the principal species are often represented, the whole consisting of two thousand genera on a thousand quarto plates, and accompanied at the same time with abridged characters of an infinity of species. The Dictionary contains a more detailed history of them, with careful descriptions, critical investigations of their synonymy, and many interesting observations on their uses, and the peculiarities of their organization. All is not original, it is true, in these two works; but the selection of figures is judicious, the descriptions are derived from the best authors, and a very considerable number of both these are to be found, which refer to species and even genera previously unknown.

It may excite surprise that M. de Lamarck, who had hitherto occupied himself with botany merely as an amateur, should so soon have been in a condition to produce such considerable works, containing representations and descriptions of the very rarest plants. The reason is, that the moment he undertook the task, he entered upon it with all the ardour of his character, occupying himself exclusively with plants, seeking them in all the gardens and in every herbarium. He spent his time among such botanists as could supply him with information, and was often in the company of M. de Jussieu, whose enlightened hospitality rendered his residence for a very long period the favourite resort of all who devoted their attention to the amiable science of plants. Whoever arrived in Paris with specimens, might be certain that M. de Lamarck would be the first to pay him a visit. His eagerness was the means of procuring him one of the finest presents he could have desired. When the celebrated traveller Sonnerat returned the second time from India in 1781, with valuable collections of objects in natural history, he imagined that all who cultivated that science would eagerly assemble round him; he could not learn at Pondicherry, or among the Moluccas, that the philosophers of this capital are too often as much engrossed as men of the world. No one appeared but M. de Lamarck, and Sonnerat in his disappointment presented him with the magnificent herbarium which he had brought with him. He likewise availed himself of that of Commerson, and the collections accumulated in the house of M. de Jussieu were generously laid open to his inspection.

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[*] Encyclopedie Methodique (Botany). The first vol., 1783, and the second, 1786, are by M. de Lamarck; the third, 1789, is by him and M. Desrousseaux, who likewise assisted with the fourth, 1795, along with MM. Poiret and Savigny; the fifth, 1804, is by MM. Poiret and De Candolle; the sixth, seventh, and eighth, from 1804 to 1808, are by M. Poiret, as well as the five supplementary vols. from 1810 to 1817.

[†] Illustrations of Genera, or an exposition of the characters of all the genera of plants established by botanists, arranged according to the sexual system of Linnæus, with figures displaying the characters of these genera, and a table of all the known species referable thereto, the description of which is found in the Botanical Dictionary of the Encyclopedie. The first vol., 1791, second, 1793, third, 1800, containing 900 plates, are by M. de Lamarck, and a Supplement by Poiret, in 1823, contains the last hundred plates.

It may likewise appear surprising, although in a different way, that M. de Lamarck has not adopted in these his great works, the more perfect modes of arrangement, the rules for which he has so accurately laid down in the preface to his Flora; and that he confined himself, in the one case, to the sexual system, and in the other, to mere alphabetical order. Such, however, were the conditions which the manager of the Encyclopædia had imposed on him, for it must be acknowledged that M. de Lamarck was still obliged to labour for booksellers, and according to their direction. This kind of labour, indeed, constituted his only resource.

The attachment of M. de Buffon, and even that of the minister, had not procured him any settled occupation; nor was any thing done for him till M. de la Billardiere, Buffon's successor, and related to M. de Lamarck's family, created for him the paltry place of keeper of the herbaria in the king's cabinet; a place of which he was continually on the point of being deprived, for strong opposition was made to its establishment, and the National Assembly was even required to suppress it, as I learn from two pamphlets which he was obliged to publish in its defence. If he obtained some years afterwards a less precarious means of support, it was only to be attained by again changing his vocation.

In 1793, the King's Garden and Cabinet were re-established, under the title of Museum of Natural History. All the superior functionaries were appointed professors, and charged with the superintendence of those departments most in unison with their preceding employments or personal studies. M. de Lamarck, being the last appointed, had to content himself with the branch not selected by the others, and was nominated to the chair relating to the two last classes of the animal kingdom, according to the Linnean division,—those, namely, which were then called Insects and Worms. He was at that time nearly fifty years of age, and the only preparatory knowledge which he possessed of this vast department of zoology, consisted of some acquaintance with shells, which he had often studied with Bruguiere, and of which he had made a small collection. But his former courage did not desert him; he began the study of these new objects with unremitting ardour. Availing himself of the aid of some of his friends, and applying, at least to all that related to shells and corals, that sagacity which a long exercise had given him in reference to plants, he laboured so successfully in this new field of inquiry, that his works on those animals will confer on his name perhaps a more lasting reputation than all that he has published on botany. Before we give an analysis of these, however, we have first to speak of other writings, which will not probably enjoy the same advantage.

During the thirty years which had elapsed since the peace of 1763, all his time had not been occupied with botany. In the long solitudes to which his restricted circumstances confined him, all the great questions which for ages had fixed the attention of men, passed through his mind. He had meditated on the general laws of physics and chemistry, on the phenomena of the atmosphere, on those of living bodies, and on the origin of the globe and its revolutions. Psychology, and the higher branches of metaphysics, were not beyond the range of his contemplations; and on all these subjects he had formed a number of definite ideas, original in respect to himself, because conceived by the unaided power of his own mind, but which he believed to be equally new to others, and not less certain in themselves, than calculated to place every branch of knowledge on a new foundation. In this respect, he resembled so many others who spend their lives in solitude, who never entertain a doubt of the accuracy of their opinions, because they never happen to be contradicted. These views he began to lay before the public as soon as he had obtained a fixed occupation; and for twenty years he continued to reproduce them in every variety of form, introducing them even into such of his works as appear most foreign to them. It is the more necessary that we should point them out, as without them some of his best writings would be unintelligible. Even the character of the man himself could not otherwise be understood; for so intimately did he identify himself with his systems, and such was his desire that they should be propagated, that all other objects seemed to him subordinate, and even his greatest and most useful works appeared in his own eyes merely as the slight accessories of his lofty speculations.

Thus, while Lavoisier was creating in his laboratory a new chemistry, founded on a beautiful and methodical series of experiments, M. de Lamarck, without attempting experiment, and destitute of the means of doing so, imagined that he had discovered another, which he did not hesitate to set in opposition to the former, although nearly the whole of Europe had received it with the warmest approbation.

As early as 1780, he had ventured to present this theory in manuscript to the Academy of Sciences; but it was not till 1792 that he published it, under the title of Recherches sur les Causes des Principaux Faits Physiques.* It reappeared in an improved order in the Memoires de Physique et d'Histoire Naturelle,* which he hastened to read to the Institute shortly after its establishment, and which he collected into a volume in 1797. According to him, "matter is not homogeneous; it consists of simple principles, essentially different among themselves. The connection of these principles in compounds varies in intensity; they mutually conceal each other, more or less, according as each of them is more or less predominant. The principle of no compound is ever in a natural state, but always more or less modified. As, however, it is not agreeable to reason that a substance should have a tendency to pass from its natural condition, it must be concluded, that combinations are not produced by nature; but that, on the contrary, she tends unceasingly to destroy the combinations which exist, and each principle of a compound body tries to disengage itself according to the degree of its energy. From this tendency, favoured by the presence of water, dissolutions result: affinities have no influence; and all experiments by which it is attempted to be proved that water decomposes, that there are many kinds of air, are mere illusions, and it is fire which produces them. The element of fire † is subject, like the others, to modification, when combined. In its natural state, every where diffused, and penetrating every substance, it is absolutely imperceptible; only, when it is put into vibration, it becomes the essence of sound; for air is not the vehicle of sound, as natural philosophers believe. ‡ But fire is fixed in a great number of bodies, where it accumulates, and becomes, in its highest degree of condensation, *carbonic fire*, the basis of all combustible substances,

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* Researches on the causes of the most important physical facts, and particularly on those of combustion; of the raising of water in the state of vapour; of the heat produced by the friction of solid bodies against each other; of the heat which becomes sensible in sudden decompositions, in effervescences, and [illegible] bodies of many living animals; of causticity, and of the taste and smell

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of certain compounds; of the colour of bodies, and of the origin of compounds and of all minerals; finally, remarks on the life of organic beings, their growth, strength, decay, and death. Paris, 1794, 2 vols. 8vo.

* Memoirs on physics and natural history, founded on reason, independently of all theory, with the exposition of new considerations on the general cause of dissolutions, on the substance of fire, on the colour of bodies, on the formation of compounds, on the origin of minerals, and on the organization of living bodies. Paris, 1797, 1 vol. 8vo.

† Memoir on the substance of Fire, considered as a chemical agent in analysis. Journal de Physique, Floreal an VII.

‡ Memoir on the substance of Sound. Journal de Physique, 16 & 26 Brumaire an. VII.

and the cause of all colours. When less condensed, and more liable to escape, it is acidific fire (*feu acidifique*), the cause of causticity when in great abundance, and of tastes and smells when less so. At the moment when it disengages itself, and in its transitory state of expansive motion, it is *caloric fire.* It is in this form that it dilates, warms, liquefies, and volatilizes bodies, by surrounding their molecules; that it burns them, by destroying their aggregation; and that it calcines or acidifies them, by again becoming fixed in them. In the greatest force of its expansion, it possesses the power of emitting light, which is of a white, red, or violet-blue colour, according to the force with which it acts; and it is, therefore, the origin of the prismatic colours; as also of the tints seen in the flame of candles. Light, in its turn, has likewise the power of acting upon fire, and it is thus that the sun continually produces new sources of heat. Besides, all the compound substances observed on the globe, are owing to the organic powers of beings endowed with life, of which, consequently, it may be said, that they are not conformable to nature, and are even opposed to it, because they unceasingly reproduce what nature continually tends to destroy. Vegetables form direct combinations of the elements; animals produce more complicated compounds, by combining those formed by vegetables; but there is in every living body a power which tends to destroy it; all, therefore, die, each in his appointed season, and all mineral substances, and all inorganic bodies whatsoever, are nothing but the remains of bodies which once had life, and from which the more volatile principles have been successively disengaged. The products of the most complex animals are calcareous substances, those of vegetables, soils, or clays. Both of these pass into a siliceous state, by freeing themselves more and more from their less fixed principles, and at last are reduced to rock-crystal, which is earth in its greatest purity. Salts, pyrites, metals, differ from other minerals only because certain circumstances have had the effect of accumulating in them, in different proportions, a greater quantity of carbonic or acidific fire.

With respect to life, the only cause of all compositions,—the mother, not only of animals and vegetables, but all bodies which occupy the surface of the earth,—M. de Lamarck yet admitted, in these his two earliest works, that all we know of it is, that living beings all come from individuals similar to themselves, but that it is impossible for us to ascertain the physical cause which has given birth to the first individual of each species.

To these two writings he added a third of a polemical description, viz. a refutation of the pneumatic theory,* in which he, in some measure, challenged the new chemists to the combat: conceiving, like so many other authors of system, that to keep silence would be to cause his system to be forgotten, and not doubting that if he could only enter it in the lists, it would obtain an easy triumph, and the public, attracted by the eclat of the dispute, would not hesitate to adopt a system of which they could scarcely otherwise be aware of the existence.

To his great regret, neither this refutation, nor his exposition, met with any reply; no one considered it necessary. He was himself, in fact, too well aware, that the whole of this edifice rested on two assertions equally conjectural; the one, that substances do not enter into combinations, unless modified in their nature; the other, that it is not reasonable to believe, that nature impresses on them a tendency to such a change. Deprived of one of these foundations, the whole falls to the ground.

We have mentioned that M. de Lamarck at this period still conceived it impossible to remount to the first origin of living beings: this was a great step yet remaining for him, and he was not long in making it. In 1802 he published his Researches on Living Bodies,† containing a physiology peculiar to himself, in the same way that his researches on the principal facts of physics contained a chemistry of that character. In his opinion, the egg contains nothing prepared for life before being fecundated, and the embryo of the chick becomes susceptible of vital motion only by the action of the seminal vapour: but, if we admit that there exists in the universe a fluid analogous to this vapour, and capable of acting upon matter placed in favourable circumstances, as in the case of the embryon, which it organizes and fits for the enjoyment of life, we will then be able to form an idea of spontaneous generations. Heat alone is perhaps the agent employed by Nature to produce these incipient organizations; or it may act in concert with electricity. M. de Lamarck did not believe that a bird, a horse, nor even an insect, could directly form themselves in this manner; but, in regard to the most simple living bodies, such as occupy the extremity of the scale in the different kingdoms, he perceived no difficulty; for a monad or a polypus are, in his opinion, a thousand times more easily formed than the embryo of a chick. But how do beings of more complicated structure, such as spontaneous generation could never produce, derive their existence? Nothing, according to him, is more easy to be conceived. If the orgasm, excited by this organizing fluid, be prolonged, it will augment the consistency of the containing parts, and render them susceptible of re-acting on the moving fluids which they contain, and an irritability will be produced, which will consequently be possessed of feeling. The first efforts of a being thus beginning to develope itself must tend to procure it the means of subsistence, and to form for itself a nutritive organ. Hence the existence of an alimentary canal! Other wants and desires, produced by circumstances, will lead to other efforts, which will produce other organs: for, according to a hypothesis inseparable from the rest, it is not the organs, that is to say, the nature and the form of the parts, which give rise to habits and faculties; but it is the latter which in process of time give birth to the organs. It is the desire and the attempt to swim that produces membranes in the feet of aquatic birds; wading in the water, and at the same time the desire to avoid wet, has lengthened the legs of such as frequent the sides of rivers; and it is the desire of flying that has converted the arms of all birds into wings, and their hairs and scales into feathers. In advancing these illustrations, we have used the words of the author, that we may not be suspected either of adding to his sentiments or detracting any thing from them.

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* Refutation of the Pneumatic Theory, or of the new doctrine of modern chemists, presented article after article, in a series of answers to the principles published by C. Fourcroy in his Chemical Philosophy; preceded by a Supplement to the theory explained in the work, entitled, Researches on the Causes of the Principal Facts in Physics, to which this forms a necessary appendage. Paris, 1830, 1 vol. 8vo.

† Researches on the organization of living bodies, and particularly on its origin, on the cause of its developments and the progress of its composition, and on that which, by continually tending to destroying it in every individual, necessarily brings on death. Preceded by a discourse delivered at the opening of the Zoological Course in the Museum of Natural History. Paris, 1802. 1 vol. 8vo.

These principles once admitted, it will easily be perceived that nothing is wanting but time and circumstances to enable a monad or a polypus gradually and indifferently to transform themselves into a frog, a stork, or an elephant. But it will also be perceived that M. de Lamarck could not fail to come to the conclusion that species do not exist in nature; and he likewise affirms, that if mankind think otherwise, they have been led to do so only from the length of time which has been necessary to bring about those innumerable varieties of form in which living nature now appears. This result ought to have been a very painful one to a naturalist, nearly the whole of whose long life had been devoted to the determination of what had hitherto been believed to be species, whether in reference to plants or animals, and whose most acknowledged merit, it must be confessed, consisted in this very determination.

However this may be, M. de Lamarck reproduced this theory of Life in all the zoological works which he afterwards published; and whatever interest these works may have excited by their positive merits, no one conceived their systematic part sufficiently dangerous to be made the subject of attack. It was left undisturbed like his theory of Chemistry, and for the same reason, because every one could perceive that, independently of many errors in the details, it likewise rested on two arbitrary suppositions; the one, that it is the seminal vapour which organizes the embryo; the other, that efforts and desires may engender organs. A system established on such foundations may amuse the imagination of a poet; a metaphysician may derive from it an entirely new series of systems; but it cannot for a moment bear the examination of any one who has dissected a hand, a viscus, or even a feather.

But his theory of chemistry and of living bodies is by no means the whole that M. de Lamarck accomplished in this way. In his Hydrogeology,* published in 1802, he advanced a corresponding theory of the formation of the globe and its changes, founded on the supposition that all composite minerals are the remains of living beings. The seas, unceasingly agitated by the tides, which the action of the moon produces, are continually hollowing out their bed; and in proportion as the latter deepens in the crust of the earth, it necessarily follows that their level lowers, and their surface diminishes; and thus the dry land, formed, as has been already said, by the debris of living creatures, is more and more disclosed. As the lands emerge from the sea, the water from the clouds forms currents upon their surface, by which they are rent and excavated, and divided into valleys and mountains. With the exception of volcanoes, our steepest and most elevated ridges have formerly belonged to plains, even their substance once made a part of the bodies of animals and plants; and it is in consequence of being so long purified from foreign principles that they are reduced to a siliceous nature. But running waters furrow them in all directions, and carry their materials into the bed of the sea, and the latter, from continual efforts to deepen its bottom, necessarily throws them out on some side or other. Hence there results a general movement, and a constant transposition of the ocean, which has perhaps already made several circuits of the globe. This shifting cannot occur without displacing the centre of gravity in the globe; a circumstance which, according to Lamarck, would have the effect of displacing the axis itself, and changing the temperatures of the different climates. If none of these things have fallen under our observation, it is on account of the excessive slowness with which these operations are carried on. Time is always necessary to account for them; unlimited time, which plays such an important part in the religion of the magi, is no less necessary to Lamarck's physics, and it was to it that he had recourse to silence his own doubts, and to answer all the objections of his readers.

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* Hydrogeology, or researches on the influence exerted by water on the surface of the terrestrial globe; on the causes of the existence of the basin

The case was no longer the same, when he ventured to make an application of his systems to phenomena capable of being appreciated by near intervals. He had soon an opportunity of

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of seas, and its successive shifting to different points of the globe; finally, on the changes which living bodies produce on the nature and condition of the [illegible]. 1 vol. 8vo. 1802.

convincing himself how far nature sometimes rebels against doctrines conceived *a priori.* The atmosphere, according to him, may be compared to the sea,—it has a surface, waves, storms; it ought likewise to have a flux and reflux, for the moon ought to heave it upwards as it does the ocean. In the temperate and frigid zones, therefore, the wind, which is only the tide of the atmosphere, must depend greatly on the declination of the moon; it ought to blow towards the pole which is nearest to it, and advancing in that direction only, in order to reach every place, traversing dry countries or extended seas, it ought then to render the sky serene or stormy. If the influence of the moon on the weather is denied, it is only that it may be referred to its phases; but its position in the ecliptic will afford probabilities much nearer the truth.*

In order to demonstrate this theory in some measure by facts, and to attract the attention of the public to it, M. de Lamarck thought it would be useful to present it under the form of predictions. He had even the perseverance to print almanacs for eleven years successively,† announcing the probable state of the temperature for each day; but it may be said that the weather took pleasure in exposing his fallacies. In vain did he attempt every year to introduce some new consideration, such as the phases, the apogee and perigee of the moon, and the relative position of the sun; in vain did he seek thereby to explain his false reckonings, and to rectify his calculations. The very succeeding season taught him, to his disappointment, that our atmosphere is subjected to influences far too complicated for mankind to calculate upon its phenomena. At last he renounced this fruitless labour, and, returning to that which he ought never to have neglected, occupied himself with the direct object of his professorship,—the history of invertebrate animals,—in which he at last found an indisputable source of reputation, and a lasting title to the gratitude of posterity. It is to him that we are indebted for the above name, *invertebrate animals*, which expresses perhaps the only circumstance in their organization which is common to them all. He was the first to use it in preference to that of *white-blooded animals*, hitherto employed; and the accuracy of his views was not long in being confirmed by observations, which prove that an entire class of these animals possess red blood. A new classification, founded on their anatomy, had been published in 1795; this he in a great measure adopted in 1797,* and substituted it in the room of those of Linnæus and Bruguiere, which at first formed the base of his course. After that period, he modified it in various ways, but without entirely changing it.† His anatomical knowledge was not of such a kind as to enable him to advance many new views; it may even be said that the general distribution of these animals into *apathetic*, *sensible*, and *intelligent*, which he at last introduced into his method, was neither founded on their organization, nor exact observation of their faculties. But what was peculiarly his own, and will continue to be of fundamental importance in all ulterior researches on these subjects, are his observations on shells and polypi, whether of a stony or flexible nature. The sagacity with which he circumscribed and characterized the genera, according to the circumstances of form, proportion, surface, and structure, judiciously selected and easily recognised; the perseverance he displayed in comparing and distinguishing the species, fixing the synonyms, and furnishing clear and detailed descriptions, have rendered each of his successive works the regulator of this department of natural history.

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* Of the influence of the moon on the earth's atmosphere; Journal de Physique, prairial, an VI.—On the variations in the state of the sky in the mean latitudes between the equator and the pole, and on the principal causes which produce them; Journal de Physique, frimaire, an XI.—On the mode of drawing up and notifying meteorological observations, in order to obtain from them useful results, and on the considerations which ought to be kept in view for this purpose; (ibid.)—On tempests, storms, hurricanes, and on the character of destructive winds; Journal de Physique, 18 brumaire, an IX.—Researches on the presumed periodicity of the principal variations in the atmosphere, and on the means of determining its existence, (ibid); read to the Institute, 26 ventose, an IX. In a note to his memoir on Sound, he promised to advance a theory of the earth's atmosphere, at which, he says, he had laboured for more than thirty years; but this was never published.

† Annual of Meteorology for the year VIII. (1800) of the Republic, containing an exposition of the probabilities acquired by a long series of observations on the state of the weather, and variations of the atmosphere, in different seasons of the year; an indication of the times when it may be expected to be fine weather or rain, storms and tempests, frost, &c.; finally, an enumeration, according to probabilities, of the times favourable for fêtes, journeys, voyages, harvest, and other undertakings in which it is of importance not to be interrupted by the weather; with simple and concise directions regarding those new measures. Paris, 1800, continued till 1810, forming altogether 11 vols.

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* See the table inserted at the 314th page of his Memoires de Physique et d'Histoire Naturelle, and the subjoined note, the only testimony he has left of the source whence he derived it. This table differs from the arrangement in question, only in establishing a class of radiarii which cannot be maintained, and in leaving the crustacea with insects, a union which he afterwards regarded as improper.

† In his system of Animaux sans Vertebres, in 1810,[1] he adopted the class of Crustacea, and created that of Arachnides, in consequence of some observations which had been communicated to him on the heart and pulmonary

[1] System of Invertebrate Animals, or general table of the classes, orders, and genera, of these animals; presenting their essential characters, and their distribution according to their natural relations and organization, after the mode of arrangement adopted with preserved specimens in the galleries of the Museum of Natural History; preceded by a Discourse delivered at the opening of the Zoological Course in the Museum, year VIII. of the Republic. 1 vol. 8vo. Paris, year IX.

sacs of spiders. In 1812, in his Researches on the Organization of Living Bodies,[1] he admits the class of Annelides, established, as he acknowledges in the 24th page, on my observations respecting their circulating organs, and the colour of their blood. In 1809, in his Philosophical Zoology,[2] he creates two classes in addition, viz. the infusoria disjoined from the polypi, and the centripedes separated from the molluscæ. In this work, also, he for the first time presented animals in the inverse ratio of their organization, beginning with the most simple.—He preserves this order and arrangement in the *Extract from his Course*, published in 1812;[3] and besides, he separates in that work the classes of animals into the grand divisions *Apathiques*, *Sensibles*, and *Intelligents*.—It is on this plan that he drew up his grand history of invertebrate animals, begun in 1815.[4]

[1] See Supp. p. 36.

[2] Zoological Philosophy, or exposition of considerations relating to the natural history of animals; of the diversity of their organization, and faculties resulting therefrom; of the physical causes which support life in them, and give rise to the movements which they execute; and those which produce, sometimes feeling and at other times intelligence, on such as are so endowed. 2 vols. 8vo. Paris, 1809.

[3] Extract from the Zoological Course in the Museum of Natural History, on the invertebrate animals; presenting the arrangement of animals, the characters and principal divisions, together with a simple list of genera. 1 vol. 8vo. Paris, 1812.

[4] Natural History of Invertebrate Animals, presenting the generic and particular characters of these animals, their distribution, their classes, their families, their genera, and the principal species; preceded by an introduction, determining the essential characters of an animal, and its distinction from vegetables and other natural bodies; and finally, an exposition of the fundamental principles of zoology. 7 vols. 8vo. Paris, 1815 to 1822. This is M. de Lamarck's capital work. A part of the 6th, and the whole of the 7th, volumes were drawn up by his daughter from his papers. In the 6th, the Mytilacés, the Malleacés, the Pectinides, and the Ostracés, are by M. Valenciennes. The first five are written by M. de Lamarck himself, assisted in the part relating to insects by the advice of M. Latreille.

It was chiefly according to his views that such as have written on the same subject, have named and arranged their species; and even at present, we should in vain seek for a more complete account of sponges (for example), of alcyons, and many other kinds of corals, than what is afforded by his *Histoire des Animaux sans Vertebres.* There is one branch of knowledge in particular to which he has given a remarkable impulse, the history, namely, of shells found in the bowels of the earth. These had attracted the attention of geologists from the time that the chimerical notion was exploded, which attributed their origin to the plastic force of a mineral nature. It was perceived that a comparison of such as belong to the different beds, and their approximation to those now living in different seas, could alone throw light on this anomalous phenomenon,—the deepest, perhaps, of all the mysteries which inanimate nature presents to our view. This comparison, however, had scarcely been attempted, or, if it were, it was made in the most superficial manner. The study had been regarded as a trifling object of curiosity. Whence do they come? Have they lived in our climate, or, have they been transported hither? Are they still in a living state elsewhere? All these important questions could not be answered but by carefully examining them one by one. The prosecution of this inquiry was the more tempting to M. de Lamarck, on account of the basin of Paris being, perhaps, the only spot in the world where such a vast number of these productions are accumulated in so small a space. At Grignon, which does not exceed a few square toises in extent, no fewer than 600 different species of shells have been collected.

M. de Lamarck entered upon this examination with that profound knowledge which he had acquired of living shells, and the excellent figures and careful descriptions which he produced, caused those beings, deprived of life for so many ages, again, as it were, to reappear in the world.*

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* Memoir on the fossils of the neighbourhood of Paris, comprising the determination of species which belong to marine invertebrate animals, and of which the greater part are figured in the collection of drawings in the museum.—This memoir, begun in the Annals of the Museum, vol. i., and continued in the subsequent volumes, was never brought to a conclusion. It was accompanied with a collection of plates of fossil shells found near Paris, with their explanation. Vol. i. 4to. Paris, 1823.

It was thus that M. de Lamarck, by resuming occupations analogous to those which first procured him reputation, at last raised for himself a monument which will endure as long as the objects on which it rests. Fortunate had it been for him if he had been able to render it more perfect. But we have already seen that he was late in devoting himself to zoology; and from the first, the weakness of his eyes obliged him to have recourse for the investigation of insects to our celebrated associate M. de Latreille, whom Europe recognises as his master in this immense department of Natural History. The clouds thickened upon him by degrees, and allowed but an imperfect glimpse of all those delicate organizations, the observation of which constituted his only enjoyment. No art could stop the inroads of this calamity, nor administer a remedy; that light, which had been so much the subject of his study, at last entirely failed him, and he passed many of his last years in absolute blindness. This misfortune was the more distressing, because it overtook him in such circumstances that he could obtain none of those means of distraction or alleviation which might have otherwise been procured. He had been married four times, and was the father of seven children. The whole of his little patrimony, and even the fruits of his early economy, were lost in one of those hazardous investments, which are so often held out as baits to credulity by shameless speculators.

His retired life, the consequence of his youthful habits, and attachments to systems so little in accordance with the ideas which prevailed in science, were not calculated to recommend him to those who had the power of dispensing favours. When numberless infirmities, brought on by old age, had increased his wants, nearly his whole means of support consisted of a small income derived from his chair. The friends of science, attracted by the high reputation which his botanical and zoological works had obtained for him, witnessed this with surprise. It appeared to them that a government which protects the sciences, ought to have been more careful to become better acquainted with the situation of a celebrated individual; but their esteem for him was doubled, when they saw the courage with which the illustrious old man bore up against the assaults both of fortune and of nature. They particularly admired the devotedness which he inspired in such of his children as remained with him. His eldest daughter, entirely devoted to the duties of filial affection for many years, never left him for an instant, readily engaged in every study which might supply his want of sight, wrote to his dictation a portion of his last works, and accompanied and supported him as long as he was able to take some exercise. Her sacrifices, indeed, were carried to a degree which it is impossible to express; when the father could no longer leave his room, the daughter never once left the house. When she afterwards did so for the first time, she was incommoded by the free air, the use of which had been so long unfamiliar to her. It is rare to see virtue carried to such a degree, and it is not less so to inspire it to that degree; and it is adding to the praise of M. de Lamarck to recount what his children did for him.

M. de Lamarck died on the 18th December 1829, at the age of eighty-five years, leaving only two sons and two daughters. The eldest of these sons occupies an important place in the Corps des Ponts et Chaussées. His place in the Institute has been given to M. Auguste de Saint Hilaire, whose travels in America have procured so many interesting plants, and which he has studied so profoundly. His chair in the Museum of Natural History, the object of which was too extensive for the exertions of one individual, has been, at the request of his colleagues, divided into two by the government; M. Latreille taking the charge of Insects and Crustacea; and M. de Blainville of all the other divisions which constituted the Linnean Class of Vermes.

On the Hindu Astronomical Tables. Communicated by the Author.

There is a very singular revival of a justly exploded opinion of the character of these tables, in the published proceedings of the Anniversary Meeting of the Royal Asiatic Society, held on Saturday 9th May 1835, appended to the journal of that Society. It is in the report of a speech, or observations, made by Sir Alexander Johnston, Chairman of the Committee of Correspondence. In that, Sir Alexander is represented as saying:— "Laloubere, a man of great research, who was sent by Louis XIV. on a mission to Siam, was the first person who in modern days brought to Europe any document shewing the nature of the Hindu Astronomical Tables. He brought to France a copy of the Siamese Table, which was a subject of a good deal of consideration to the astronomer Cassini. The French subsequently brought to Europe the Hindu Astronomical Tables found at Krishnapuram, those found at Naraspur, and finally, those found at Trivalore, a place twelve miles to the west of Negapatnam; these three places are all situated in the southern peninsula of India. The astronomical tables found at Trivalore, are supposed to have been formed upon observations made 3000 years before the Christian era, a fact which Bailly and Playfair both conceived to be proved, as they found, upon calculating back to the time when these tables were supposed to have been formed, that the situation of the heavenly bodies must have been precisely such as described in these tables. Bailly and Playfair also remark, that the Hindus could not have formed these tables without an extensive knowledge of geometry, and of plane and spherical trigonometry, or of some substitute for them."

Few persons at all conversant with the recent progress of science and literature, are ignorant of the opinions of Bailly and Playfair on this subject, and of the advantage that some persons took of them to propagate, with great zeal, a total scepticism respecting the authenticity of the true records of mankind, which we possess in Europe, the acknowledged chronology of these not agreeing with such an early advance of science in India. It was in vain that Mr Jones had early demonstrated the true nature of the Hindu Tables, in opposition to the opinion of Bailly and Playfair. By a certain class of writers, they were held forth as a faithful record of actual observations, for many years after the death of Bailly. In the Edinburgh Review, especially, as if some writer in that work had taken the opinion of Bailly and Playfair under his special protection, we had a series of papers taking the accuracy of that opinion for granted, down to the very time when the question was finally set at rest in Laplace's Système du Monde.

The writer of this note has no means of corresponding with Sir Alexander Johnston or the Royal Asiatic Society, and begs to take advantage of the Edinburgh New Philosophical Journal, the pages of which, he has no doubt, will, with its customary liberality, be lent to correct an error in science, to direct public attention anew to Laplace's demonstration of the real nature of the Hindu Astronomical Tables, by quoting it here from the Système du Monde. Sir Alexander Johnston, as we learn from a note appended to the report of the Anniversary Meeting, has been requested to reduce his observations to writing; and it is to be hoped may correct the oversight, in the published report, of the lucid statement of Laplace. It is the object of the Royal Asiatic Society, as we learn from Sir Alexander Johnston's speech, to diffuse European learning and science in India; but the young gentlemen whom we send out thither with that view, would be less stimulated to their noble task, and especially could feel no interest in the introduction among the Hindus of that which is infinitely more valuable than human learning and science, and that is our revealed Theology and Ethics, were they to leave our shores infested with any degree of the scepticism appended to the opinion of Bailly and Playfair, and go into regions where they could have little opportunity for correcting that erroneous opinion.

What follows is copied from Harte's translation of the Système du Monde, vol. ii. pp. 220, 221, 222 (Dublin 1830). The demonstrations are too varied, complete, and consistent, to leave any doubt that the Hindu Tables are the result, not of observation, but of erroneous calculation backwards to anterior time.

" In Persia and India," says Laplace, " the commencement of astronomy is lost in the darkness which envelopes the origin of these people.

" The Indian tables indicate a knowledge of astronomy considerably advanced, but every thing shews that it is not of an extremely remote antiquity. And here, with regret, I differ in opinion from a learned and illustrious astronomer, whose fate is a terrible proof of the inconstancy of popular favour, who, after having honoured his career by labours useful both to science and humanity, perished a victim to the most sanguinary tyranny, opposing the calmness and dignity of virtue, to the revilings of an infatuated people, of whom he had been once the idol.

" The Indian tables have two principal epochs which go back, one to the year 3102, the other to the year 1491, before our era.

These epochs are connected with the mean motions of the sun, moon, and planets, in such a manner, that, setting out from the position which the Indian tables assign to all the stars at this second epoch, and reascending to the first by means of these tables, the general conjunction which they suppose at this primitive epoch is found. Bailly, the celebrated astronomer, already alluded to, endeavours, in his Indian Astronomy, to prove that the first of those epochs is founded on observation. Notwithstanding all the arguments are brought forward with that perspicuity he knew so well to bestow on subjects the most abstract, I am still of opinion, that this period was invented for the purpose of giving a common origin to all the motions of the heavenly bodies in the zodiac. Our last astronomical tables being rendered more perfect by the comparison of theory with a great number of observations, do not permit us to admit the conjunction supposed in the Indian tables; in this respect, indeed, they made much greater differences than the errors of which they are still susceptible, but it must be admitted that some elements in the Indian astronomy have not the magnitude which they assigned to them, until long before our era; for example, it is necessary to ascend 6000 years back to find the equation of the centre of the sun. But, independently of the errors to which the Indian observations are liable, it may be observed, that they only considered the inequalities of the sun and moon relative to eclipses, in which the annual equation of the moon is added to the equation of the centre of the sun, and augments it by a quantity which is very nearly the difference between its true value and that of the Indians. Many elements, such as the equation of the centre of Jupiter and Mars, are very different in the Indian tables from what they must have been at their first epoch.

"A consideration of all these tables, and particularly the impossibility of the conjunction at the epoch they suppose, prove, on the contrary, that they have been constructed, or at least rectified, in modern times. This also may be inferred from the mean motions which they assign to the moon, with respect to its perigee, its nodes, and the sun, which, being more rapid than according to Ptolemy, indicate that they are posterior to this astronomer; for we know, by the theory of universal gravitation, that these three motions have accelerated for a great number of ages."

On a Species of Beroe found on the North-east Coast of Ireland.

By Robert Patterson, Esq. Treasurer to the Belfast Museum. Communicated by the Author.* With figures on Plate I.

The necessity of separating the species of Beroe, furnished with long ciliated tentacula, from such as are destitute of these organs, was so apparent, that Dr Fleming was induced to form them into a distinct genus, under the term *Pleurobrachia.* The only British species yet included in this division is the *Beroe pileus.* It was first added to the British Fauna by Montagu in 1812. It is mentioned by Scoresby in his Arctic Regions, and several interesting particulars respecting it are recorded by Audouin and Milne Edwards. For the principal part of our knowledge respecting it, we are, however, indebted to the excellent paper published by Dr Grant in the Transactions of the Zoological Society.

During the spring and summer of the present year, 1835, I have taken in considerable numbers on the north-east coast of Ireland, a species of Beroe, furnished with tentacula, but differing in many particulars from the *Beroe pileus* described by Dr Grant. As the high character as a naturalist which this gentleman has so justly attained, precludes the imputation of inaccuracy on his part, I am compelled to believe that the Irish species is distinct from the *B. pileus,* and consequently that it has not hitherto been recorded as British.

While the observations I have now to bring forward were in progress, I resided in the immediate vicinity of the small seaport town of Larne, in the county of Antrim. My lodging was situated on the small peninsula termed the Corran,† and nearly midway between the two stations, whence ferry-boats ply to the opposite peninsula of Island Magee. Through the narrow channel, across which these boats are continually plying, the tide runs with great rapidity into Larne Lough. Hence I had, by means of the ferry-boats, an easy mode of taking, at all hours during the day, the small *Medusæ* and *Crustacea*, which the flow of the tide placed within reach of a small canvass towing net. As the Beroes could thus with facility be procured, and were to me highly attractive, my sitting-room, for between two and three weeks, was never without some of them. They were kept in glass jars, the water in which was changed twice each day. With this precaution only, a few individuals were kept alive and vigorous for five days, and might with similar care have continued so for a much longer period. In general, however, after a few hours' confinement, or at most after a couple of days, they were poured back into the sea, and their place supplied by other individuals. In this way I had a constant succession of beroes newly taken, possessing in perfection their locomotive powers, and forming a subject for ceaseless admiration and remark.

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* The principal part of this paper was read by the Author before the Natural History Society of Belfast on the 3d of June 1835, and several living specimens of the animal exhibited.

† This word in the Irish language signifies "Reaping Hook," to which implement the little peninsula has a striking resemblance in form.

The size of the animal is from two to seven lines in length, and about one-third less in breadth, exhibiting a regular oval form. Many individuals are, however, globose, or more nearly resembling the shape of an orange. In every other respect they appear alike, and I attribute the difference rather to a contractile power possessed by the animal, than to any permanent dissimilarity in form. The body is transparent and colourless, with the exception of a deep coloured conspicuous pink line towards the centre of the stomach, which line becomes bifurcate as it advances upwards.

It is furnished with eight bands placed at regular intervals, and extending at each side about three-fourths of the distance from the mouth to the anus, but approaching more nearly to the latter. To these bands the cilia are attached, and, as the band is less broad at either extremity than in the centre, the cilia exhibit a corresponding decrease. Dr Grant, in speaking of the cilia of *B. pileus*, states, that there are about forty in each band, and that " they are not single fibres, but consist of several short straight transparent filaments, placed parallel to each other in a single row, and connected together by the skin of the animal, like the rays supporting the skin of a fish." In the Irish species of beroe now under consideration, the number of cilia is much less than is here stated. In some individuals they amounted to only fifteen, and in none did they exceed twenty-seven. The several filaments of each cilium are not connected together by any membrane. They are totally distinct, very numerous, tapering, and slightly recurved towards the extremity. Along each band a cord or slight ridge extends, dividing it longitudinally into two equal parts. The filaments on each band consist, therefore, of two parcels totally detached. The parcels, which correspond in situation, move in general at the same time. The motion, however, is not always simultaneous; each portion seems to possess a separate and independent power of motion, and hence, while one portion is vibrating in the usual manner, the other may be seen moving more slowly, or perhaps perfectly at rest. The length of the cilia at the outer edge in each row, is precisely the same as the distance from the one row to that immediately above; it is greater, however, towards the centre, and hence the base of the cilia in the middle of the row is overlapped by the extremity of those beneath.

The stomach appears to consist of two membranous plates joined at their edges, and capable of being extended, so as to inclose an almost circular space. In general, however, they are so nearly together that they present very different appearances in different positions. The upper edge of each membrane is divided into two semi-circular lobes, and these are constantly varying, both in the extent to which they are protruded and that to which they are distended. It is seldom they are porrected to their full extent, but, when so, they produce so great a change in the oval form which the animal generally presents, that they make its outline appear like a miniature representation of one of those old fashioned bottles which we see in the pictures of the Flemish, the short neck of the vessel being generally uppermost.

The mouth and œsophagus, as Dr Grant has observed, are wide, and the stomach extends to the centre of the body. These parts, I am inclined to think, are capable of considerable distension. My attention was directed to this circumstance on the first evening that any beroes were taken. With them I found in my net a large number of *crustacea* of a bright green colour, and from a line to a line and a half in length, which, on subsequent examination, have proved to be some undescribed species of cyclops. They were placed in a glass jar with the beroes. In the course of an hour afterwards, when candles were brought into the apartment, I noticed that several of the beroes had one of these little animals in the cavity of the stomach, the bright green colouring of the cyclops rendering them particularly obvious, and contrasting beautifully with the crystalline transparency of the body in which they were inclosed. Although the length of these crustacea was equal to the one-fourth of the average length of the bodies of their devourers, some of the latter were not content with even this quantity of food, for two beroes were noticed in each of which two of the cyclops were contained.

If, however, the beroes prey upon the small crustacea, they in turn furnish a supply of food to medusæ more powerful than themselves. On the 12th of May I took a small medusa of the genus Callirhoë, but of a species undescribed by Lamarck, and placed in the glass vessel with it a beroe which had been taken at the same time. While the latter was swimming round the glass with that lively and graceful movement for which it is so remarkable, it came in contact with the filiform tentacula attached to the arms of its companion. The arms instantly closed, and the beroe was a prisoner. I endeavoured to separate them, and for this purpose moved them about, by pushing them with a camel-hair pencil, but without effect. In about half an hour afterwards, when I again observed them, they were asunder, the beroe swimming about, and the cilia of its bands vibrating as briskly as usual. It had not, however, escaped uninjured from its captor. The Callirhoë had taken from the body of the beroe "a huge half moon, a monstrous cantle out." In fact the portion thus removed occasioned a vacancy which extended transversely across three of the bands, and longitudinally for about the one-third of its entire length. The being who had suffered this mutilation seemed, however, quite unconscious of its misfortune, moved about in every respect as before, and for four days, during which I afterwards kept it, seemed to possess all its powers in unimpaired activity.

To this instance of apparent insensibility to pain may be added one illustrative of the extent to which the principle of vitality, or of vital irritability, seems embued throughout every portion of its frame. On one occasion two beroes were taken after a storm, with some of the cilia abraded, and other parts of the body shattered and even torn. Any of the cilia, however, which were attached to these mutilated parts, retained all their former mobility unimpaired. The most damaged of these beroes was then cut with a pair of scissors into several pieces, and each part exhibited in its cilia the same undiminished rapidity of movement. One of these portions was again subdivided into parts so minute as to possess only one, or at most two cilia on each, yet no change in the ceaseless motion of these extraordinary organs took place. Thirty-three hours after this minute subdivision, several of them were vibrating as usual; and, at the expiration of forty-two hours, the two cilia belonging to one fragment shewed undiminished activity.

On one occasion one of the beroes died during its confinement in the glass jar. I then took the body, made in it a longitudinal incision, and placed it in the small concave glass belonging to the microscope. In the course of a short time, the substance of the body had melted down into a homogeneous watery mass. Soon, however, the warm air of the sitting-room caused some of the fluid particles to evaporate, and the residuum gradually assumed greater opacity and consistence, displaying in a confused manner the two tentacula, and different bands of cilia. These, when the evaporation was complete, remained as if delicately painted in distemper colours on the glass, and were removed by a touch of the finger as completely as if they had never appeared in any more animated state of existence.

Although, from this circumstance, it is obvious that the quantity of solid matter which enters into the composition of their bodies, must be extremely trifling, they possess a greater degree of firmness and consistency than is generally supposed. Frequently have some of them dropped from my net into the boat when about transferring them to the glass vessels in which they were kept; and, at such times, I have invariably lifted them in my fingers, and placed them with their companions, without their having received any apparent injury. If the finger be pressed against one recently dead, the beroe will not, by such a pressure, be changed into a broken and shapeless mass. It will, on the contrary, by its smoothness and elasticity, slide from beneath the finger. My observation, therefore, does not confirm the remark of Blainville, " à peine est il touché, qu'il est brisé et réduit en morceaux."—(*Manuel d'Actinologie*, p. 150.) A portion of albumen enters into the composition of these bodies; for the beroe becomes more opaque, and in some degree coagulated, when thrown into alcohol or into boiling water.

While engaged in making some observations on the appearance exhibited under a lens by the cilia when in motion, I found to my surprise that the light afforded by a pair of candles was more favourable for such a purpose than even that of the sun. The rays of that luminary were colourless as the body of the beroe, and passed through it as they would have done through a piece of crystal. The yellower and weaker rays of the candle had a different effect. Impeded in their course, they coloured the animal with a tint different from that of the sea-water by which it was surrounded, and exhibited in darker shadings the portions of the body which were less translucent than the adjacent parts. Numerous dark, irregular, and somewhat parallel lines, were thus brought into view, extending the entire length of each band. The imperfect instruments I possessed prevented any observations as to the functions performed by these parts in the economy of the animal.

All the ferrymen, sailors, and boat-builders about the shores of Larne Lough, and the passengers, who at all hours during the day were passing to and from Island Magee, seemed utterly unacquainted with the existence of such a creature as the beroe. On many occasions, they expressed in the strongest terms their admiration at its appearance and movements; and it was no unusual circumstance for half a dozen of individuals to surround me on my landing, and ask permission to see my captures. It was at such times, when the beroes had just been taken from the water, that they exhibited in the highest perfection their locomotive powers, and displayed in the bright sunshine a splendid iridescence of colouring caused by the action of their cilia. As they wheeled onwards, rising and falling at pleasure, and creating in their course the glory by which they were encircled, they seemed indeed, though in a sense different from that of the poet,

——— "Gay creatures o' the element,
That in the colours of the rainbow live."

The variety of their movements constituted one of their principal charms. Sometimes they would ascend from the bottom of the jar to the surface of the water, with a slow and regular movement, resembling that of a balloon, and descend at the same rate of progression. Again they would rise more rapidly, turn the mouth downwards, and descend with equal rapidity. At other times, without rising or falling, they would revolve on the transverse axis of the body, a movement of the reality of which the language of Lamarck implies a doubt. Then abandoning all these modes of progression, they would revolve on their longitudinal axis, holding the body vertical, and in this position twirl round and round the glass. When the movements of the body are thus varied, how great must be the variety of motion in the cilia by which the body is propelled! Never for more than a second or two do the cilia cease to vibrate. Even then it is not a total cessation, but a slower and alternate movement that is exhibited. The cilia on one or two continuous bands would then remain stationary, while the adjacent ones on either side would move. Then those which had been still would be set in motion, and those which had been moving would remain still. No regular succession of movement was observed, but some portion of the bands of cilia was kept constantly in motion. Hence it may fairly be inferred that they are organs of respiration as well as of locomotion, and that the term "Pleurobrachia" applied to them by Fleming, is as applicable as "les Ciliogrades" of Blainville. If water moving in elastic tubes along the base of the cilia be the power by which they are propelled, it is obvious from the preceding observations, that the animal can direct the water into any particular band, and regulate at pleasure the celerity of its undulations.

The tentacula of these animals were, next to the cilia, the most attractive of their parts. These organs were not always apparent, but remained inclosed in the body of the animal. Among the first thirty-five beroes taken, two only exhibited the tentacula, although subsequent examination shewed that they were not wanting in any individual. They were seldom displayed immediately after the beroes had been taken from the net, nor while the glass vessel in which they were kept was crowded by the number it contained. When, however, not more than five or six were placed there, the tentacula were thrown out to their st extent, and were occasionally above six times the longest diameter of the body of the beroe. In the specimen of *B. pileus*, which came under the observation of Dr Grant, they were about four times the length of the animal, which would appear to be about the average size. The same author remarks, " They extend from two curved tubes, placed near the sides of the stomach, which pass obliquely downwards and outwards, to terminate between two of the bands, at some distance above the mouth." " These tubes have a sigmoid form, and are shut and somewhat dilated at their upper extremity." In the Irish species the tubes are not curved in the form described, and their external orifice is at some distance, not from the mouth, but from the anus. The tentacula appear to be much alike in both. " Along their whole course they present," says Dr Grant, " minute equidistant filaments, extending from their lower margin, which coil themselves up in a spiral manner, and adhere close to the tentacula, when they are about to be withdrawn into their sheaths or tubes." The filaments were in some individuals not less than half an inch in length, and of a delicate pinkish colour. To my eye they never presented the appearance exhibited in the engraving illustrative of Dr Grant's paper. In some instances they were more numerous than from that engraving we would suppose, for even so many as fifty may occasionally be reckoned on a single tentaculum. Most accurately has Dr Grant remarked, " The tentacula are often thrown out from their tubes to their full extent by one impulse, and the slow uncoiling of the slender serpentine filaments from their margin, is then very beautiful: when coiled up they appeared like very minute tubercles along the side of the tentaculum." Of course, in particular points of view, they presented a moniliform appearance; and sometimes, while the filaments on the upper half of the tentaculum were seen under this aspect, those in the lower half were like delicate hairs or cilia, waving from the edge. In this respect, however, they were incessantly varying, and the tentacula, at the same time, were continually assuming new aspects, being retracted either separately or together, and thrown out in the same diversified manner. It is scarcely possible to convey, by any description, an idea of the beauty and diversity of their forms. They seem endued with exquisite sensibility, which, however, is not always equally delicate. At times the slightest touch will cause a tentaculum to be drawn back into its tube, with a sudden jerk: at other times it is apparently unfelt. The beroes never seemed poised, or supported in the water by their tentacula. In one instance, however, they were extended to the bottom of the vessel, where they seemed to act as suckers, and formed fixed points, whence the animal rose and fell at pleasure, and appeared as if moored by these delicate and novel cables; the mouth being retained in the usual erect position.

The ovaries of the specimen described by Dr Grant, "consisted of two lengthened clusters of small spherical gemmules, of a lively crimson colour, extending along the sides of the intestine and stomach." In above 200 of the Irish beroes examined by me, these crimson gemmules were totally wanting. At first I thought their absence might be accounted for by the difference of season at the time the observations were made, Dr Grant's being in September, while mine took place in May. This idea proved to be erroneous: for I had opportunities of examining parcels of beroes taken in Larne Lough, on the 3d of June, the 14th, 22d, and 24th of July, the 20th of August, and the 14th of September, and in no instance did they differ, in any particular, from those observed the preceding spring.

Lamarck observes, "Les beroës sont tres-phosphoriques; ils brillent pendant la nuit, comme autant de lumières suspendues dans les eux; et leur clarté est d'autant plus vive que leurs movemens sont plus rapides." Dr Macartney's notice of *B. fulgens* (*Phil. Trans.* 1810, p. 264), shewed the luminous property belongs to some of the species found on the British coast. It does not, however, seem to prevail universally; at least in all the observations I have been enabled to make, I have never in even one instance been able to detect its presence. The species of beroe taken at Larne is also found in the Lough at Belfast; and specimens precisely identical have been shewn to me by Robert Ball, Esq. of Dublin, which were taken by him in August last, in the bay outside of Kingston Harbour. They are, therefore, found diffused over a considerable range of the eastern coast of Ireland.

The numerous particulars now mentioned, especially those relating to the number and structure of the cilia, and the form of the sheaths of the tentacula, seem to me to warrant the conclusion that the Irish species is distinct from the *Beroë pileus*, and consequently that it is now for the first time, added to the British Fauna. I do not venture at present to bestow on it any specific name, as it may perhaps be found to be one of the six species of tentaculated beroes, included by Blainville, in his *Manuel d'Actinologie*, under the generic term "Cydippe." This I am unable to determine, as that author gives no description of the species, but refers to the publication of M. Eschscholtz, at Berlin, 1829, a work to which I have not had access.

The present paper cannot be concluded better than by some extracts from the diary kept during my residence at Larne, shewing the kind of weather that prevailed during the time the beroes were captured. These extracts will demonstrate, that, in this country, the month of May, "the rosy-footed May" of the poets, exhibits occasionally but few of its poetical attributes.

From the 28th of April to the 2d of May, the wind continued northerly, and extremely stormy. Some pieces of the wreck of a large vessel were thrown ashore near Ballygally Head. May 2. Some light rain fell between 1 and 2 o'clock in the day: the evening was fair and unusually mild and calm: visited Island Magee, and took thirty-five beroes. The time usually occupied in crossing the ferry, in fine weather, is from eight to ten minutes, or from fifteen to twenty minutes going over and returning. May 4. Extremely warm, the mirage along all the little bays very perceptible: crossed the ferry twice, taking a considerable number of beroes each time. May 5, 11 A. M. A very strong gale from the south or S.SW: a small cutter aground on the edge of the channel, and a brig on the opposite shore: by timely assistance both vessels were got off in safety: the gale increased to a storm, accompanied by heavy showers, which continued with little intermission from 1 o'clock until 9 P. M. May 6. The storm has abated, although the wind still continues high, and there are frequent showers: crossed the ferry, using two nets both in going and returning: took no crustacea and only six beroes: two of these were dead; two much broken, and two uninjured. May 7. As the day advanced, the wind, which had fallen considerably, became still more tranquil, and the evening was perfectly calm: crossed the ferry, and took about forty beroes. May 8. The evening calm but cold, and terminated towards 7 o'clock in thick drizzling rain: I had at that hour been sailing or rowing in the harbour for three hours; yet during that time took only nine beroes. May 9. The day very stormy, with almost incessant showers: in crossing the ferry and returning, not one beroe was taken: rowed for a short time in the evening, using both my nets, with no better success. May 11. The morning fine, with light breezes from the west and WSW., and a gentle rippling undulatory motion over the surface of the water: crossed the ferry, but did not get even one beroe: went on board the schooner Supply, to the light-house on the North Maiden Rock: a part of the stores only were landed, when the captain was compelled, by the increasing violence of the wind, to desist from the attempt. May 12. Blowing hard: crossed the ferry taking six beroes. May 13. The morning fine: towards noon a fresh breeze from the south and south-east: sailed for above an hour; got but one beroe. May 14. Last night has been extremely stormy; to-day the wind continues very high from the north-east. May 15. The storm last night was greater than any we have yet had: to-day it still continues, accompanied by frequent rain: nineteen small vessels are now riding at anchor in the harbour, the greater part of which have run in to seek shelter from the gale. May 16. The storm is abated: the little vessels are drying their sails, and some of them preparing for sea: crossed the ferry, and took thirteen beroes, all of them dead from the violence of the late gale. May 19. Towards the evening of the 16th, the wind again rose, until at length the ferry-boats were unable to cross, and the boatmen refused to attempt to do so, although many times the usual fare was offered as an inducement. During the entire of the 17th and 18th, this state of things continued, and all intercourse with Island Magee was suspended. This day, when the wind and rain had abated, and the boats had once more begun to ply, my net was sent across the ferry, attached to the stern of the boat. Its contents, on subsequent examination, proved to be a small number of beroes, all of them dead. From the facts mentioned in these extracts, two inferences respecting the beroes may be drawn,—that they are never taken in abundance except during fine weather; and that their absence from the surface during storms, is not sufficient to protect them from serious and even fatal injury.—(The figures of this Beroe are represented in Plate I. See last page of this Number.)

Reply to Dr John Davy's Remarks on Certain Statements by Mr Faraday, contained in his "Researches on Electricity," in the Edinburgh Philosophical Journal for October 1835. By MICHAEL FARADAY, D. C. L., F. R. S., &c. &c. *

THE secretary of the Royal Society having mentioned to me the preceding paper, I requested a sight of it, that I might, as soon as possible, correct any error in the papers to which it referred, and of which it might make me conscious; and having read it, I am induced to hope the present note may accompany Dr Davy's observations.

I do not know that I have any right to suppose Dr Davy generally does not understand me in my papers, and yet something of this kind must have occurred; for instance, the new law of conduction referred to in my Fourth Series, is not even now evident to him, and therefore I think I cannot have erred in supposing Sir Humphry Davy unacquainted with it. The *law* is, that *all substances* decomposable by the pile are in the fluid state conductors, and in the solid state nonconductors, of the electricity of the voltaic battery (393, 394, 404, 407, 413, 505, 676, 679, 697, &c.). The more careful examination of this law in other parts of my printed Researches shews that no bodies but electrolytes have this relation to heat and electricity, the few exceptions which seem to occur being probably only apparent (690, &c.). That the title of *law*, therefore, is merited, and that this law was not known to Sir Humphry Davy, are, I think, justifiable conclusions, notwithstanding Dr Davy's remarks. As to Priestley's results with the electric machine, they really have nothing to do with the matter.

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* Reprinted, from the London and Edinburgh Philosophical Journal, at request of Dr Faraday.—EDIT.

I have said that Sir Humphry Davy spoke in general terms. "The mode of action by which the effects take place is stated very generally, so generally, indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing essentially from each other, yet all agreeing with the statement there given (482)." In this and other parts of what I have written (483—484), which Dr Davy quotes, he thinks that I have been deficient in doing justice, or in stating Sir Humphry Davy's "hypotheses" correctly.

Dr Davy for my word "general," substitutes "vagueness." I used *general* in contradistinction to *particular*, and I fear that vagueness cannot, with propriety, stand in the same relation. I am sure that if Sir Humphry Davy were alive, he would approve of the word I have used; for what is the case? Nearly thirty years ago he put forth a *general* view of electro-chemical action, which, as a general view, has stood the test to this day; and I have had the high pleasure of seeing the Royal Society approve and print, in its Transactions of last year, a laborious paper of mine, in support and confirmation of that view (1834, part ii. page 448). But that it is not a particular account is shown, not merely by the manner in which Sir Humphry Davy wrote, but by the sense of his expression, for, *as Dr Davy says*, "he attached to them no undue importance, believing that our philosophical systems are very imperfect, and confident that they must change more or less with the advancement of knowledge." and what have I done but helped with many others to advance what he began; to support what he founded?

That I am not the only one, as Dr Davy seems to think, who cannot make out the precise (or, I would rather say, the particular) meaning of Sir Humphry Davy in some parts of his papers, may be shewn by a reference to Dr Turner's excellent Elements of Chemistry, where, at page 167, of the fifth edition, the author says: "The views of Davy, both in his original essay, and in his subsequent explanation (Philosophical Transactions, 1826), were so *generally and obscurely* expressed that chemists have never fully agreed, as to some points of the doctrine, about his real meaning. *If* he meant that a particle of ygen or free chlorine is in a negatively excited state, then

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rans. 1826, p. 390, Edin. New Phil. Journ., Oct. 1835, p. 33.

his opinion is contrary to the fact, that neither of these gases affect an electrometer," &c. &c. Having similar feelings, I thought that I was doing Sir Humphry Davy far more justice in considering his expressions as general, and not particular, except where they were evidently intended to be precise, as in the cases which I formerly quoted (483—484)*.

Again, Dr Davy says, " What can be more clear than this; that my brother did not consider water as essential to the formation of a voltaic combination?" &c. If this be so clear, how happens it that Mr Brande, in the last edition of his Manual, vol. i. p. 97, says that " Sir Humphry Davy further remarks that *there are no fluids, except such as contain water*, which are capable of being made the medium of connexion between the metals of the voltaic apparatus;" and Mr Brande's observation is, " This, however, appears to me to admit of doubt?" How happens it also that Dr Ure, in giving his eloquent account of Sir Humphry Davy's discoveries†, uses the very same words as those I have quoted from Mr Brande, adding, ' It is probable that the power of water to receive double polarities, and to evolve oxygen and hydrogen, is *necessary* to the constant operation of the connected battery?" I ought, perhaps, rather to ask, How could Sir Humphry Davy use such words, and mean what Dr Davy wishes to be considered as his meaning? Why, *there can be no doubt that if I had proved that water was the only substance that could perform these duties, Dr Davy would have claimed the discovery for his brother.*

As I cannot impute to Dr Davy *the intention of doing injustice*, the only conclusion I can come to is,—that the language of Sir Humphry Davy is obscure even to his brother, who thinks

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[*] I may be allowed to quote in a note a passage from one of Mr Prideaux's papers, of the date of March 1833; I was not aware of it when I wrote in answer to Dr Davy. Mr Prideaux says, " Sir Humphry Davy's theory assumes that 'chemical and electric attractions are produced by the same cause; acting in one case on particles, in the other on masses: and the same property, under different modifications, is the cause of all the phenomena exhibited by different voltaic combinations.' A view so comprehensive, embracing every modification of chemical as well as electrical action, seems to include the other two, *and every one that has been or can be attempted* on the subject. But what it gains in extent it wants in distinctness."—*Lond. and Edin. Phil. Mag.*, vol. ii. p. 215.

[†] Chemical Dictionary, *art.* ELECTRICITY.

it perfectly clear; so obscure, indeed, as to leave on his mind the conviction of a meaning the very reverse of that which it bears to Mr Brande and Dr Ure. Thus Dr Davy puts his seal to the truth of Dr Turner's observation[*] by the very act of denying it.

What makes the matter still more remarkable is, that Dr Davy charges it upon me as a fault, that I, and *I alone*, have said what he denies in words, but proves in fact; whereas *I have not said* it, and others have.

If Sir Humphry Davy's meaning is thus obscure to his brother, I have no right to expect that mine should have been rightly taken; and therefore it is that I suspect, as I said before, that Dr Davy, generally, does not understand me in my papers.

That "probably a dozen precise schemes of *electro-chemical action* might be drawn up differing from each other, but all agreeing with Sir Humphry Davy's general statement," is no exaggeration. I have in the very paper which is the subject of Dr Davy's remarks quoted six: 1. that of Grotthus (481); 2. of Sir Humphry Davy himself (482); 3. of Riffault and Chompré (485); 4. of Biot (486); 5. of De la Rive (489); and 6. my own (518, &c.). These refer to modes of decomposition only; but as I spoke in the passage above quoted of "electro-chemical action," in reference to chemical effects and their cause generally, I may now quote other particular views. Volta, Pfaff, Marianini, &c., consider the electricity of the voltaic pile due to contact alone. Davy considered it as excited by contact, but continued by chemical action. Wollaston, De la Rive, Parrot, Pouillet, &c., considered it as of a purely chemical origin. Davy, I believe, considered the particles of matter as possessing an inherent electrical state, to which their chemical properties were due; but I am not sure of his meaning in this respect. Berzelius, according to Turner, views them as being naturally indifferent, but having a natural appetency to assume one state in preference to another[†], and this appears to be the theory of M. Fechner also[‡]. Again, electro-chemical phenomena have been hypothetically referred to vibrations by Pictet, Savary, myself,

[*] And to that Mr Prideaux's also.

[†] Turner's Elements, 5th edit., p. 167.

y Journal of Science, vol. xxvi. p. 428.

and others. Now, all these views differ one from another; and there are, I think, a dozen of them, and it is very likely that a dozen more exist in print if I knew where to look for them; yet I have no doubt that if any one of those above could be proved, by a sudden discovery, to be the right one, it would be included by Dr Davy, and, as far as I can perceive, by myself also, in Sir Humphry Davy's general statement. What ground is there, therefore, for Dr Davy's remarks on this point?

In reference to another part of Dr Davy's observations, I may remark, that I was by no means in the same relation as to scientific communication with Sir Humphry Davy, after I became a Fellow of the Royal Society in 1824, as before that period, and of this I presume Dr Davy is aware. But if it had been otherwise, I do not see that I could have gone to a fitter source for information than to his printed papers. Whenever I have ventured to follow in the path which Sir Humphry Davy has trod, I have done so with respect and with the highest admiration of his talents, and nothing gave me more pleasure in relation to my last published paper, the Eighth Series, than the thought that, whilst I was helping to elucidate a *still obscure* branch of science, I was able to support the views advanced twenty-eight years ago, and for the first time, by our great philosopher.

I have such extreme dislike to controversy, that I shall not prolong these remarks, and regret much that I have been obliged to make them. I am not conscious of having been unjust to Sir Humphry Davy, to whom I am anxious to give all due honour; but, on the other hand, I feel anxious lest Dr Davy should inadvertently be doing injury to his brother by attaching a meaning, sometimes of particularity and sometimes of extension, to his words, which I am sure he would never himself have claimed, but which, on the contrary, I feel he has disavowed in saying "that our philosophical systems are very imperfect," and in expressing his confidence "that they *must change more or less with the advancement of science.*" On these points, however, neither Dr Davy nor myself can now assume to be judges, since with respect to them he has made us both partisans. Dr Davy has not made me aware of any thing that I need change; and I am quite willing to leave the matter as it stands in the printed papers before scientific men, with only this request, which I am sure, before-hand, will be granted, that such parts of Sir Humphry Davy's papers and my own as relate to the subject in question, be considered both as to their letter and spirit, before any conclusion be drawn.

ROYAL INSTITUTION, JANUARY 9. 1835.

An Account of Professor EHRENBERG'S *more recent Researches on the Infusoria.* With a Plate.

IN some recent numbers of this Journal, we have endeavoured to make our readers acquainted with the important researches of Professor Ehrenberg respecting the *Infusoria*, chiefly through elaborate analyses of his memoirs by Drs M. Gardiner and Sharpey, in the volumes for 1832–33. Since that date, this indefatigable naturalist has, with unwearied assiduity, been prosecuting his investigations, and has given an account of them in a folio volume which has been lately published at Berlin.* We shall now endeavour to present our readers with a short epitome of the most important of his additional discoveries.

This new work is divided into three parts. The first is devoted to the refutation of the hypothesis which maintains the existence of a primitive organic substance; the second consists of observations on divers points of the anatomy and physiology of the *Infusoria*; and the third contains a description of three new families, thirty-one new genera, and 135 new species of *Infusoria*. It is of the second part only that we shall here present an account.

I. *On the Existence of a Pharynx and of Teeth in the Polygastric Infusoria.*

In his earlier works, M. Ehrenberg had fixed on the want of a pharynx as a distinctive character of the class Polygastrica; whilst this organ, accompanied with teeth, was found conspicuously to exist in the class Rotatoria; but, by certain improvements in his microscopes, he has more recently succeeded in distinctly discovering teeth in the *Loxodes cucullulus* (Kolpoda cucullulus, Müller). This discovery he published in his second memoir on microscopic beings; and his attention being thus anew directed to the point, he prosecuted his researches, and has since found six species of polygastric Infusoria, which clearly exhibit a distinct pharynx and a masticating apparatus.

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* Organisation in der Richtung des Kleinsten Raumes. Dritter Beitrag. Von C. G. Ehrenberg. Folio, 11 Plates. Berlin 1834.

Of these six species only one was previously known, the *Loxodes cucullulus;* the five others had not previously been observed or recorded; though the Professor observed them in very great numbers in the spring of 1832. According to the situation of the mouth and vent, these Infusoria ought to be placed in two different families, viz. in the *Echelida* and *Trachelina;* and if their external organization is considered, it will be found they belong to three different genera. Their teeth may be more easily observed than those of the Rotatoria, in which the animal must be destroyed before the number is determined. The pharynx is placed on the surface of the body, and often somewhat projects from it. As we might expect, M. Ehrenberg regards the dental apparatus so important, that, by its presence or absence, he determines species, and also uses it for the distinguishing of genera. Thus, he has separated, to make distinct genera therewith, the *Loxodes cucullulus,* and all the other neighbouring species which are provided with a masticating apparatus, although the rest of their organization would associate them with the genera *Loxodes, Holophrya,* and *Bursaria,* in which no such apparatus exists. The names he has given to those Polygastria which have teeth, are *Euodon cucullulus* (Loxodes cucullulus), *Nassula ornata, N. elegans, N. aurea, Prorodon niveus,* and *P. compressus.* The three species united in the genus Nassula are very interesting, in many points of view, and were previously wholly unknown.

As to their form and connections, the teeth of the polygastric Infusoria differ from what is seen in the Rotatoria. The teeth of the Kolpoda and the Bursaria present the form of a cylinder, or of a slender and long hollow cone. (See Plate I. fig. 2, &c. in which *a* is the crown of the teeth). They are placed at the entrance of the mouth; they cover its interior surface, and are disposed in series which closely approximate each other. Their proportional length exceeds that of the rotatory animals. Their anterior extremity is truncated, and their indentations are always more solid and distinct before than behind, where they are more or less indistinct and mossy; this is also remarked in the teeth of the Rotatoria, which, in their turn, present a close analogy to those of the Entomostracea (Daphnia, Cyclops). In pressing the animal between two plates of glass, so as to bruise the soft parts, the teeth remain, and are very distinctly visible, which proves that they are more solid than the other parts.

The number of teeth is greater in this class than in the Rotatoria. None of the polygastric infusores which possess them have less than 16; and, in the larger, as, for example, in the *Prorodon compressus*, more than 30 have been discovered. Professor Ehrenberg has in many instances given the precise number. Thus, *Euodon cucullulus* has 16; the *Nassula ornata* 26; the *N. elegans* 22; the *N. aurea* 20; and the *Prorodon niveus* has more than 20. The extreme minuteness of these teeth, and their position in very close cylindrical groups, very much increases the difficulty of observation, and especially that of the determination of the number, which, indeed, cannot be done, except when the animal is so placed that the opening of the mouth directly faces the observer. In all other positions, he can see but a part of the teeth, because there are always some which are covered by the others.

The pharynx of these Infusoria has less play in the act of deglutition in this class than in the Rotatoria. It often happens, that when the animal is vibrating its rotatory cilia, it leaves its mouth open, and without action, and then the nourishment which happens to be drawn in, enters into the cavity indifferently, and the animal does not move its jaw at all. But when the little creature wishes to swallow larger morsels, then it previously masticates them. In this case, the buccal cylinder first enlarges itself at its orifice for the reception of the nourishment; at that time it is narrow farther down, but as the morsel proceeds, it contracts behind, and dilates before it. During this movement, the mouth itself is often quite shut; nor is it rare to witness all these movements executed, when no large morsel at least has been taken.

An essential difference between the teeth of rotatory and polygastric animals consists in this,—that in the first they are attached to the bottom of the mouth on the pharynx, and act laterally one upon the other; whilst, in the Polygastrica, they rather resemble in their arrangement a purse-net, the form of which they also assume. Whilst the mouth is open, monadina of a considerable size can easily pass between the teeth down into the stomach, but the narrowing of a dental cylinder at the bottom of the mouth does not allow them to come out again so easily, even if the mouth be open. Perhaps the contractions which are sometimes remarked at the anterior part of the cylinder, apparently without any distinct object, are the result of the sensation which the animal experiences at the bottom of the mouth, when the animalcules it has swallowed endeavour in this way to make their escape.

Finally, it may be observed, that, on one occasion, when M. Ehrenberg was dividing a *Nassula ornata* and a *N. elegans*, he observed the existence of a second and new dental apparatus; and that, in an individual of the first species, he observed an irregularity in the original one which appeared to be preparatory to a longitudinal division of the teeth. The regeneration of the whole teeth of the mouth, a phenomenon truly rare in the animal series, is very common among the Infusoria, with whom a tendency exists to multiply, by a spontaneous division. The Infusoria lacera, as soon as the posterior portion is separated from the rest of the body, produces afresh a new anterior portion, provided with a mouth, teeth, &c. M. Ehrenberg mentions his having seen a great number of Infusoria of a primary or normal formation, which he had examined during the day, divide themselves transversely during the following night, and, next morning, all the posterior parts had each produced a mouth full of teeth, which were perfectly organized. Many others were not entirely divided into two parts: he gave an unremitting attention to these, and found that the development of the parts for a time deficient made the most rapid progress; to such an extent, that he calculated that the whole of the division, and the formation of twenty teeth, might be effected in the space of two hours. (See Pl. I. fig. 2.)

II. *Concerning a System of Internal Organs, simple, double, or multiple, very irritable, which may be discovered in the Polygastrica, and which, perhaps, are the male Sexual Organs.*

Though Professor Ehrenberg had very often minutely examined the *Paramecium aurelia*, one of the best known Infusores, and had often witnessed its mode of reproduction, it was nevertheless not till a late period that he ever observed a great double organ, placed in the interior of the body, the knowledge of which, he conceives, is not more important for the elucidating the anatomical structure of this animal, than for the science of physiology in general. Its existence, he believes, evidently proves, that, besides the organs of digestion, of respiration, and the female genital apparatus, there exists in these Infusores other organs still, which can belong neither to the vascular nor nervous system, but which probably constitute a portion of the generative apparatus, by which the animal impregnates itself. For a long time, he had observed in the bodies of the greatest number of Polygastrica isolated vesicles, which often contracted rapidly and disappeared, and then after a time dilated anew. But as these small vesicles often exactly resembled those which were filled with nutritive matter, he regarded them as stomachs, which the animal had perhaps alternately filled and emptied, and he moreover believed that perhaps all the vesicles of the stomach had this power. Thus it was, that frequently in the figures which accompanied his earlier works, transparent vesicles may be perceived placed at the side of the intestine, and which at other times are not so indicated. In the *Trachelius anas*, they invariably appeared so large, that he was led to consider them as particular stomachs, remarkably voluminous, and filled with water, and he always thus represented them. After the observations we have alluded to at the commencement of this paragraph, the Professor latterly directed his attention to these vesicles, endowed with this singular faculty of sudden contraction and dilatation, and to his surprise discovered that they existed to the number of three or more, though usually to the number of two only, in a fixed and determinate place of the body of the animal.

That he might the more accurately study these organs, he took a certain number of the *Paramecia aurelia*, and subjected them to pressure between two glass-plates, at the same time taking care to place between them some threads of Confervæ, to prevent their too close approximation: he thus forced these little creatures to remain stationary, and flattened them a little, without bruising them. In this way, he speedily succeeded in covering eight conduits or canals, which shot in rays from these two vesicles towards all parts of the body; they enlarged themselves by degrees when the vesicles contracted, and then contracted and disappeared when the vesicles dilated. Every one of these canals presented a swelling or bulging out at its base, near to the vesicles. These two organs resembled two small transparent Aphiures, which had been enclosed in the bodies of the Paramecia; they were all alike in all the specimens.—(See Pl. I. fig. 6, *c*.)

At a still later period M. Ehrenberg employed another mode of observation, which was still easier than that which we have just described. He placed on an object-glass a drop of water which contained a great number of these infusores, and then removed as much of the liquid as he could; so that the little beings crowded in the midst of fluid could no longer swim freely. Placed in this situation all these infusores became larger in consequence of the softness of their bodies, and exhibited in the clearest way possible the contractions and dilatations of their two great organs. He has observed these contractile organs, in every respect alike, in twenty-four species which he enumerates, belonging to genera of three different families; but with these details we shall not trouble our readers.

At the same time we remark, that these organs, both as it regards their number, or the situation of the central part of the apparatus, present essential differences in the twenty-four species in which they were found. Thus in six of them, and we shall only name the *Parameeium aurelia*, there are found to be two central parts of this system, one of which is in the middle of the anterior part of the body, and the other in the posterior part. All these infusores, too, with one exception, are known to divide transversely and spontaneously into two portions, and in these cases each part preserves one of the central points of this apparatus; and this twofold organization appears to have a relation to the division which has previously been mentioned. Sometimes also, the professor has observed that at certain epochs, there were four of these central points in many of these infusores; whilst at other times, there were only two in individuals of the same species. When there were four, there were always two in each half of the body; and all the infusores which resent this number are capable of dividing themselves transversely and longitudinally, in such a manner, that after this division, each quarter of the body preserves a central part. It is especially the *Paramecium aurelia* above named, which often presents this appearance. The *Paramecium kolpoda* presents two contracting vesicles, but they are placed near to each other, and near also to the middle of the back. The *Euodon cucullulus* has three vesicles, two of which are situate upon the two sides of the dental cylinder, and the third in the posterior part of the body, near the dilatation of the intestinal canal close to the vent. It has been remarked, that this little animal also frequently divides itself in both ways, that is to say, both longitudinally and transversely. In some others a single vesicle is placed in the anterior part of the body, in some it is in the middle, and in others it is in the posterior part.

The relative situation and the number of these organs often differs in the different species of the same genus. Thus the *Bursaria vernalis* exhibits two, whilst the *B. leuca* and the *B. flava* possess only one, and the *B. spirigera* shews none conspicuously. M. Ehrenberg also remarked that he had often unavailingly sought for the organ in many of these beings for a very long while, but when once found it always appeared very distinctly. They often remain for a long time in a state of contraction, and they are then quite invisible; so that observations must be often and patiently repeated, and their non-existence must not be admitted incautiously.

We must also observe, that Professor Ehrenberg seems to have made another important discovery in the organization of these infusores, which consists in the presence of a round organ, less transparent than those we have been just considering, and which is placed near the central contractile vesicle. In the *Euodon cucullulus* this organ is of an oval shape, of a dull white colour, of considerable size, and placed in the middle of the abdomen. A body in every respect analogous exists in all the individuals of the *Nassula elegans.* Its position is somewhat oblique in these two species. In the *Nassula ornata* and the *N. aurea* it appears more globular, and is equally near to a central vesicle. In these our species he has distinctly perceived this organ, and more recently in the *Paramecium aurelia* also. He succeeded in this by feeding the animal abundantly in some coloured material, and then this colourless, oval, and transparent body could be detected without difficulty.

In prosecuting his researches into the nature of these hitherto unknown parts, M. Ehrenberg began by examining if in the groups of infusores which were most nearly allied to those in which he found these bodies, he could find similar organs; and he commenced with the Rotatoria. The gradual contractions and dilatations which are performed by the central organ of the radiated vascular apparatus in the Paramecia, occurred to him as an excellent peculiarity to assist him in his researches. He had a long time previously discovered a vascular organ quite as contractile in the posterior part of the bodies of a great many of the Rotatoria; and this led him to compare it with the organ in question, although it did not communicate with the radiating canals having a central point. He had described and drawn this organ in detail when he dissected the *Hydatina senta.* It is in intimate and very distinct connection with the testicles; and he concluded that its function is to subserve the function or excite the activity of the internal genital members of these hermaphrodite animals. It was on this account that he then named it the ejaculatory muscle of the male seminal fluid.

In the rotatory animals, the ovary appears to be reduced to two cornea, and to produce only a few eggs at a time. In the Polygastrica, on the contrary, the granular matter which is supposed to be eggs, as they have been noticed to come from the vent of the *Kolpoda cucullulus* in the normal state, is spread throughout the whole body, and especially surrounds the intestines and the vesicles. In judging from this great extent of the ovaries, and from these differences in their organization, it would appear very probable that there are also differences in the form and distribution of the male seminal organs, and in the contractile organ which accomplishes the hermaphrodite fecundation. The mass of eggs being very large and very distinct in the Polygastrica, it is very probable that the male genital organs are equally so. Besides, such an organization accords perfectly with the great and rapid fecundity of these animals. On the other hand, the simplicity of the ejaculatory organs of the Rotatoria agrees very well with the fact, that these infusores are not capable of self-division; whilst in the Polygastrica, which exhibit many contractile organs, the power of self-division exists. Perhaps the contractile organ being twofold in these last, is only the commencement or preparation for the division of the body which is speedily to be effected.

Hence M. Ehrenberg concludes, that these contractile and vesicular organs, which are provided with radiating canals, and which are observed in the polygastric Infusoria, perform the function of fecundation, which is accomplished in the interior of the body by their conducting the seminal matter. The opaque body also, which is mentioned above, and which in many species is placed in the middle of the abdomen, he supposes to be the testicle. He moreover supports this view by comparing the convulsive movement of the vesiculæ seminales of the Mammiferæ, and recognising in the Infusores similar contractions of the organ just described, agreeing perfectly with the functions assigned to them.

It can scarcely be supposed that these contractile organs, provided with radiated canals, can be considered as respiratory organs, or as hearts, because their movements are too slow, and it is notorious that the motions of the heart, and the circulating fluids, are more rapid and more uniform in small animals than in great ones. Any one may be satisfied that the pulsations of the heart in the Daphies, and the circulation in the Distomes and the Planaries, are much more rapid than the movements we have been considering. Before it could be admitted that these organs were the respiratory apparatus, it would be previously necessary more distinctly to establish the vascular system in the Infusores. Although traces of a very delicate vascular system have been observed in the Paramecia, it is clear there can be no discussion at present concerning the circulation in this class of animals, since the existence of canals in which this circulation might take place has not yet been determined.

Having demonstrated the existence of these contractile organs, M. Ehrenberg well remarks, that there was no necessity for his going further, or drawing any conclusions at all. At the same time, as the reproductive powers of the Polygastrica, as he had previously demonstrated by experiments, are so prodigiously great, he conceives it warrants him in drawing this inference; and that he can scarcely deceive himself when he admits that these beings possess a generating apparatus, which is developed to a great extent. Hence he concludes, that the organs in question are concerned in the act of reproduction. The existence of ovaries admitting of no dispute, and the rotatory animals of a certain size being unquestionably hermaphrodites, he contends that the organs of the Polygastrica now under consideration represent the male genital organ in these animals, as, in the Rotatoria, the contractile vesicles undoubtedly accomplish analogous functions, for they closely adhere to organs which have precisely the form and situation of male seminal organs in the Daphnia and Cyclops. Hence he considers that he has demonstrated the high probability, at all events, of the existence of two sexual organs (male and female), in the polygastric Infusores.

III. *Concerning a violet or very deep blue coloured liquid, which is found in the Intestines of the Polygastrica, and concerning the particular organs which secrete it.*

As mentioned in former memoirs, Professor Ehrenberg had regarded as analogous to the pancreas two glands, considerable in size, which, in the Rotatoria, are found placed at the commencement of the intestinal canal, immediately under the œsophagus. He at that time pointed out their relations, and stated that they existed probably in the whole of these animals. If all this be true, as their situation and their connection with the intestine, as well as their glandular structure, would lead to infer, then the pancreatic fluid in all the Rotatoria hitherto ascertained, judging from the colour of the organs themselves, is either clear and colourless, or whitish, milky, and mixed-like. For a time in the Polygastrica, he was not able to discover any traces of these organs, though so distinct in the Rotatores; but more lately, he has discovered many species of Infusores, which exhibit in the interior of their bodies a liquid of an exceedingly beautiful violet colour. This liquid flows into the intestinal canal, colours its contents, and with them is expelled from the body. Professor Ehrenberg discovered this liquid especially distinct in two species. In a third, belonging to a neighbouring genus, he very distinctly observed the traces of the secretion of a liquid which was more red than that just alluded to. He designated these three species by the names of *Nassula elegans*, *N. ornata*, and *Bursaria vernalis.* He could best distinguish this apparatus in the first of these; and the details which now follow are principally supplied by that species.

In all young specimens of the *Nassula elegans*, which are neither too pale nor too much shrunk, may be discovered a beautiful violet spot, placed in the anterior and back part of the body, opposite to the dental cylinder of the mouth. This part, though higher than the mouth, cannot be considered as the head, because the intestine is ramified within it; it is to be considered as a protuberance placed there, which, however, leaves the general form of the animal, regular and cylindrical. It is irregular, almost square, and as large, frequently, as the ridge itself. This spot is formed of a great number of small violet globules, very unequal in size, or rather of a great number of colourless vesicles filled with a violet-coloured liquid. Proceeding from this point, a simple canal may be seen, resembling a string of pearls, running along the back, in which the violet coloured matter is carried towards the posterior part of the body. It is only in the last third of the body, that a direct union seems to exist between this canal and the vesicles or stomachs of the animal; for, in this point, the violet colour of the liquid shews itself altered, and mixed with a little foreign matter. In truth, there may be often remarked the same nutritive substances, such as fragments of oscillatories and vacillories, in this part of the canal, and in the stomach. In all these infusores, the violet matter is found to be discharged at the vent, placed at the posterior part of the body, either pure, or mixed with nutritious matter, or with the excrementa. The violet vesicles in the posterior part of the body have always appeared in greater proportion than any other matters: the evacuation of the former into the latter, and their consequent augmentation, has been often witnessed. At first sight it appears as if these little beings had received as nourishment this violet-coloured matter; but this is not the case; and, wholly peculiar in its nature, it proceeds from themselves alone. It would appear that the vesicular mass which is placed in the posterior part of the neck in the *Nassula elegans*, is the secretory organ of this liquid; for vessels have never been observed to terminate in this mass, and all the organs surrounding it are transparent and colourless. Sometimes these vesicles were the only visible ones in the body, and only in two instances, out of more than a hundred, have they been found colourless.

Regarding the *Nassula ornata*, and the *Bursaria vernalis*, which are the other two species which frequently secrete this fluid in great abundance, the Professor has not succeeded in discovering any fixed points in which the secretion was effected. This, perhaps, has arisen from the secretory organs of this fluid so much resembling in form, size and situation, the other internal organs of the animal, that they could not be discovered. In the *Nassula ornata*, the secretion of this liquid is especially abundant, more abundant even than in the *Nassula elegans:* in it the violet vesicles are very clearly distinguished from the stomach, which is filled with greenish or brownish-yellow matter, and they are equally distinguished from the ova, which are of a bright green colour. In the *Bursaria vernalis* its secretion is much less abundant. It is perceived only at the time when the water in which these little animals swim is evaporated, and when their bodies, being no longer supported, swell; or when a slight pressure is applied to their frames without crushing them. When treated in either of these methods, isolated stomachs or vesicles may be discovered, which are filled with fragments of oscillatores, or of vacillaires, and these surrounded with a violet, reddish or brownish liquid.

M. Ehrenberg has remarked, that this violet liquid, which is somewhat viscid, and almost oily, possesses a dissolving power, which it exerts over all the bodies which it envelopes; for he has often noticed, that in the stomachs which contained much of it, the minute fragments of the oscillatores were always found much altered and discoloured, divided or decomposed. He also mentions another property which he had remarked. In bruising one of these Nassulas, he has observed, that this beautiful violet colour immediately disappeared as soon as it came into contact with water, although the oily drop which it formed did not mix with the surrounding mass. It was evident, then, that the water exerted a chemical agency over the liquid. This experiment was often repeated, and was witnessed by many, and the decoloration was always effected in the same way. That more certainty might be obtained regarding this phenomenon, many of these animals were placed in small globules of oil, and apparatus, or branchiæ, somehow modified almost to infinity. Every point of the skin more especially destined to respiration, and still more the branchiæ, ought especially to present the primitive movements, that is to say, oscillations. In the lowest links of the animal kingdom, among the Protozoaires, the Infusores exhibit in the circles of the delicate cirri with which they are provided, the most convincing example of the disposition just stated. The filaments, fine and transparent as glass, which are found in the Leucophrys, the Kolpoda, the Vorticella, the Rotiferes, and others, and which usually occasion by their excessively rapid oscillations, the optical illusion of a turning-wheel, belongs wholly to the series of these organs." After this opinion, founded upon the principles of modern philosophy, the existence of a heart and vasculatory system may not be always necessary for the carrying on of respiration; and the rotatory movements of the Infusores would be a mode of perfect respiration, since they effect in the ambiant liquid certain movements of attraction and repulsion.

After reviewing these various hypotheses, and more particularly the last, M. Ehrenberg rejects the whole of them, and for this reason, especially, that he distinguishes expressly between simple movements and respiration, properly so called, and because, in the infusores, at least, they are two distinct processes. Moreover, he cannot believe that there is in this phenomenon any particular influence exerted by the ambiant medium upon the body of the animal, and, without this, respiration, in any way, could not take place. But instead of entering more into detail concerning previously admitted doctrines on this point, M. Ehrenberg proceeds at once to certain positive observations, which he has been happy enough to make. Many years previously he had remarked a local vibration in certain points of the interior of the bodies of many of the Rotatoria, and especially in the *Brachionus urceolaris*. And latterly, after having informed himself concerning the direction of the internal muscles of the body, it occurred to him that these vibrations were effected by several portions of the muscular substance, which induced him at the time not to attach any great importance to their examination. It was when maintaining these views that he alluded to this discovery in his first treatise on the infusores, in 1830, in which he employed the following terms:—"I have often observed small local vibratory movements performed in different parts of the bodies of Rotatoria, which I regard as muscular movements; I have also observed from time to time a fluctuation amongst the organs in the abdominal cavity." It was these observations which led him to the subsequent discovery of the system of an organ which appears to exist in all the rotatory animals.

A new species of the genus *Notommata*, of a large size, furnished him, in the spring of 1832, with an opportunity of completely convincing himself that these local movements in the interior of the body are not merely muscular vibrations, but that they are performed by particular organs, which are symmetrical, and which occupy a fixed point. In examining the *Notommata centrura* from behind, he distinctly remarked seven of these vibratory points in the right side, and six in the left. (See Pl. I. fig. 10, *b. b.*) They were never at rest, and their position was symmetric, opposite to each other, at determinate distances. Accurate observations have demonstrated that these points are small peculiar organs, provided with a tail, the form of which is similar to that of a note of music, which are put into vibration by three small vesicles or folds bulging out at their extremity. In consequence of the motions of the animal, it was also observed that these organs floated freely in the abdominal cavity by their bent extremity, whilst they were attached by their tail to two organs in form of a bent club, which M. Ehrenberg, from his researches upon the *Hydatina senta*, regards as seminal organs. He also concluded that these last organs possessed a vascular system; for, at the time of the local dilatations of the body of the animal, a certain number of filaments, which may be vessels, and which are free and loose, may be very distinctly observed.

The idea which occurred to our author, when he for the first time observed these small vibratory organs, was, that he perceived a vascular system executing the movements of pulsation; but soon he reflected there was much difficulty in admitting so great a number of hearts in an animal which exhibited no traces of a circulation; for a time, then, he remained in suspense, and proceeded to the examination of other rotatory animals. In a figure of the *Notommata collaris*, he had previously marked four of these vibratory points, situated regularly opposite to each other. This led him to infer that this little animal, and the *Brachionus urceolaris*, in which, for the first time, he had observed these vibratory motions, would manifest these organs the most distinctly; and this he found to be the case. They are less distinct in the *Hydatina senta*, the *Cycloglena lupus*, and a new and very large species resembling the *Notommata centrura*, which is distinguished by lateral cirri, not unlike wings; and which he has named *Notommata copeus*. Since the *Brachionus urceolaris* belongs to the cuirass-rotatory animals, and since the organs in question are also found in the *Euchlanis macrura*, their existence is demonstrated in the two orders of the class of Rotatoria, and in the three families of the Hydatina, the Euchlanidia, and the Branchionia.

The number of genera in which these organs have been hitherto discovered is six, and the number of species is eight. At the same time, M. Ehrenberg believes that these are not the only species which possess these organs, and the more so as they are often very difficult of discovery. It is thus he remarks that he did not succeed in discovering them in the *Hydatina senta* which he had examined more than a hundred times, and with the greatest possible attention, after the greatest possible care, and having published the details of its organization in plates, which were engraved in the *Symbolæ Physicæ*. All these researches previously made had not supplied him with the slightest suspicion of their existence; whereas now he can easily demonstrate them to any one who would wish to see them. Very probably their discovery presents similar difficulties in all the rotatoria, and these must be overcome before we can hope to see the organs.

More lately M. Ehrenberg found an opportunity of examining the *Notommata clavulata*. (See Pl. I. figs. 8 and 9.) Considering the size, and the great transparency of this animal, he always thought that the organs in question should exist in it. After some labour he found them, and in extremely interesting relations. They are not adhering to the seminal organs as in the other infusores, but they are attached to a distinct vessel, which is free, very thick, and very transparent. He counted more than 'rty small clubs attached to this vessel, in a simple line, and on one side only, which made it resemble the combs which scorpions carry under the belly. These clubs and their vessels are so small, and so clear, like the purest crystal, that it is almost impossible to perceive them, except when the animal is in motion; but when once seen, they may always be distinguished very plainly. Only one of these organs has hitherto been discovered. As the small clubs are very numerous and very compact, it is probable that no more exist.

It is expected of him who discovers an organ, that he will give an account of its relations with the other parts of the animal; and the method which our author took to accomplish this object was the following:—The observations to which we have been just alluding, led him to return to an external organ, which has been formerly mentioned, and of which he then did not know the function, viz. the spur which is found at the neck of a great number of the Rotatoria. At first he had supposed that this spur was an existing organ of the sexual system, because it resembled in its situation and form the penis of the univalve Mollusca. But even in his second dissertation, he had demonstrated in detail that this organ has no connexion with the internal sexual parts: and this it was which induced him to substitute the term spur for that of clitoris. Now, when he connects his former observations concerning the fluctuation in the abdominal cavity of the Rotatoria, to what he has established concerning the crooked organ which is placed upon the neck, and also to that which he has established concerning the vibratory organs attached in a line to the sides of the interior of the body, he cannot hesitate to consider all these parts as forming a very distinct respiratory apparatus. He regards the spur as a syphon, or respiratory tube, and believes that the periodic transparence, the extension and subsequent subsiding of the body, which almost regularly takes place in all the Rotatoria, is the result of the introduction of water into the internal cavity of the body. The fluctuations which he had observed in the interior of the body would then become the moving of this water. When the internal cavity of the body of these Rotatoria is thus filled, all the internal organs appear isolated, so that their limits are seen very distinctly; but the water being evacuated, which may be excellently seen in the Hydatina senta, they, on the contrary, approach to each other, their limits are confounded, and the outer covering of the body becomes folded. Viewing all these phenomena, he does not fear to state that the small vibratory moving bodies which are placed in two longitudinal series in the internal cavity of the body, are to be regarded as *internal branchiæ*, and he means to maintain this opinion till more detailed observations shall assign to them some other function in their economy. The multiple hearts, which are found, according to Prevost, in the Chirocephales, still demand particular attention. They are not placed in two series, but simply one behind the other; and they may rather be compared to the dorsal vessel of insects than to the organ now under review. Besides, the whole form of these Entomostracea resembles more one of the larvæ of those insects which copulate previous to the time of their development, as happens in the Orthopteres and the Hemipteres.

V. *On the Nervous System of the Infusoria.*

Professor Ehrenberg is apprehensive lest he should be charged with temerity for admitting the existence of an isolated nervous system, forming an apparatus similar to that existing in vertebral animals and in insects, in a class of animals which have been supposed to be without any structure at all, according to many ancient observations, and many recent theories. Up to this period, his remarks on the nervous system of the Infusores have only been general, and he has treated particularly only of the *Hydatina senta;* here, however, he favours us with some details to prove that this system must be recognised as existing in the minute beings which are now occupying our attention.

In most animals in which a nervous system is generally admitted, the nerves are usually distinguished by their whitish colour; the muscular fibres, and the vessels are of a colour somewhat red or yellowish; the tendinous fibres are bluish, and the cellular tissue is transparent and clear like water. But these characters are not sufficient when we are called to distinguish, with certainty, nervous filaments which are exceedingly delicate; and doubts exist even when the filaments are of a considerable size. Another character, which is often very decisive, consists in bluish zigzags which present themselves during the contractions of the nervous fibres in the nerves; but this character is only sufficient for nerves of considerable bulk, and sometimes it may be mistaken for the fibres of tendons, as, for example, for the tendons of the toes of frogs. Experiments with the galvanic pile cannot go beyond a certain degree of minuteness. The only means which have been hitherto discovered, whereby to arrive at certainty concerning the nature of a slender filament which is suspected to be a nerve, is to trace its course to the branch whence it proceeds, and thence to its junction with the spinal cord, or the brain, or with a distinct ganglion, or, finally, to recognise it penetrating into one of the organs of sense. Unfortunately, microscopic researches upon nervous matter are not far advanced; and it almost appears that the transparency of the nervous matter in microscopic animals, is an insurmountable obstacle to our cognizance of the existence of this substance, and of its structure.

But in spite of these unfavourable circumstances, M. Ehrenberg never became a convert to the opinion so generally adopted, that the nervous matter is intimately mixed up with, and not separated from, the body even of the most irritable of the Infusores; on the contrary, he conceives that certain organs in these animals are the brain and nerves. This opinion is founded upon three considerations, *1st*, The possibility of demonstrating the existence of organs resembling in form the brain and nerves; *2d*, Their arrangement within the body; and, *3dly*, Their distinct communication with the eyes.

As to the first of these considerations, the whole of his observations upon the special organs of the Infusores, and their comparison with the organs of superior animals, have demonstrated to him that the number of the organs, or the sum of organization, was remarkably equal in these two kinds of animals. It would certainly be ridiculous and inadmissible to speak of nerves and of a nervous system in animals which had no other organs, or which presented mere indistinct traces of them, as unhappily has sometimes been done. But our author has discovered in the Rotatory animals, *1st*, A system of organs of nutrition, with all its details; *2d*, A double sexual system, observed in all its development; *3d*, A more extensive vascular system, the existence of which is at least highly probable; and, *4th*, Distinct muscles and internal ligaments, having an arrangement and a power corresponding to the external organs of locomotion.

Besides these organs, he has also discovered in these animals, others whose forms and functions appear to have no relation with the systems just enumerated. These apparently superfluous organs are of two kinds, the one of a glandular form, the other filamentous. The substance of the former, seen through the microscope, appears to be very minutely granular; and that of the latter is also granular, or rather homogeneous and transparent. In both of them no internal cavity can be distinguished, though many of them present a diameter quite large enough for their display. Two of these bodies, of a globular or cylindrical form, are placed immediately behind the œsophagus, at the commencement of the intestine, and, where there is a stomach, immediately behind it; they are large and easily recognised in all the Rotatoria. M. Ehrenberg regards these as two glands, because they are intimately united with the intestines, without being cœcums, for they are never filled with nutritive matter, and they always follow the movements of the intestine. Both are attached by their anterior extremity to the internal surface of the abdominal cavity, by means of a ligament, small like the finest filament; sometimes they shew a vesicle in their interior. At one time he imagined these two glands, whose situation is exactly that of the two cœcums in the Daphnia, to be the pancreatic gland. He never witnessed them filled with coloured nutritive matter, whilst the cœcums in the Daphnia are very soon coloured along with the intestine, as may be easily seen by experiments made with indigo.

M. Ehrenberg regards the small bodies situated round the pharynx in the rotatoria (see Pl. I. figs. 8 and 9) as nervous ganglions, because they are not intimately united, and because they do not necessarily belong to any of the systems previously enumerated. Many of these bodies send forth slender filaments, which in their arrangement present no analogy, either with the dichotomic course of vessels, nor with the muscles; neither do they contract as the muscles do, when the animal is in motion, and do not present any thickening during their contractions. Nor does it appear that these filaments can be vessels, although in similar circumstances these also remain in a passive state;

for, were they vascular, the movement of a liquid would be perceived in the larger of these filaments, on account of their considerable diameter, and of the granular substance which they inclose. If, on the other hand, many of these knots which are placed near the œsophagus were to be regarded as performing the function of salivary glands, this view could not be taken respecting those which send distinct filaments into other parts of the body, such as the mouth of the pharynx. Besides, the intestinal glands which have been already noticed, are themselves very considerable organs for the function of salivation. In those animals in which they are placed upon the intestinal canal, as in the Brachiones, they may rather be named salivary than pancreatic glands; but when the stomach is not distinct from the intestines, as in the Hydatina, they at the same time perform the functions of both pancreas and salivary glands.

Finally, there is found in the middle of the body of certain of the rotatoria, small isolated knots (see Pl. I. figs. 8 and 9, *g g*), which are freely suspended between the long filaments; they are very delicate and simple, and give origin to other slender filaments, or more accurately, several of these filaments, and sometimes only two, unite in these bodies. These small and free organs, which are always placed in the same spot, have distinctly the form of ganglions and nerves; and they move in a passive manner, with the movements of the muscles.

The second consideration which confirmed him in the belief that there were nerves in the infusoria, was the particular arrangement of the bodies we have just been considering. These large knots—those most like nervous or cerebral ganglions—are found placed exactly round the œsophagus and near the mouth; and it is well known that it is precisely in this place that the well determined nervous ganglions are found in other animals, and especially in the Entomostracea, the Mollusca, and in worms. The rest of the body presents a nervous distribution, simple, and also by rays intermixed with ganglions, agreeing very well with what we should expect.

The discovery of a direct communication between the medullary knots situated on the neck, near the œsophagus, and the constant red points which are commonly found in the same situation, appeared to him especially to settle the point, and to prove the nervous nature of these organs. As early as 1831, in his second dissertation, he had demonstrated that these red points are the eyes, and he now supports this opinion by many new proofs. In his former dissertation, he remarked that these points always maintained the same situation, and that their form, their colour, and their position, exhibited the strongest analogy with the eyes of the young Entomostracea, of the genus Cyclops, animals which had always been regarded as provided with eyes. Though directed by this analogy, he, moreover, supported his views by the granular structure of the substance of the pigment, and by the size of the nervous knot upon which the double eye of the Cyclops is placed, an organ of which no notice had previously been taken. But it will be still easier to perceive this approximation by comparing these parts with the more delicate eyes of the Daphnias. All the known species of Daphnias have two kinds of eyes, as flies have. The large compound eyes, of a black colour, are moved by four muscles, according to M. Strauss, and by eight according to our author.

Near these eyes a cylindrical prolongation, round before, may be very distinctly perceived, which arises from the brain, and which is to be considered as the optic nerve; it is continued forwards by two fine threads, which enter directly into the middle part of the eye; the optic nerve reposes upon a large knot of medullary matter; from this a second and thick elongation arises, which becomes thinner in its progress towards the middle of the forehead. Immediately behind the termination of this prolongation, a reddish or blackish spot is seen, of a round or elongated form, which in its colour and substance resembles the eyes of the Rotatoria. This spot has not been observed by Jurine; M. Strauss has exhibited it in an imperfect manner in the *Daphnia pulex*, and it has been more accurately observed by Messrs Schœffer and Gruithuisen. The eyes of the Cyclops have no resemblance to the composite eyes of the Daphnias, whilst in a striking way they resemble the small points or eyes of these same Daphnias, which may be called simple eyes in comparison of the others, which are larger, and provided with a great number of facets.

Schœffer had previously observed the several parts of the brain of the Daphnies, better even than Jurine. But the former of these observers had given to the brain too many portions, as the latter had described too few; for Schœffer erroneously regarded the feelers of the females, which are inclosed in the lower and truncated part of the forehead, as a third part of the brain, whilst Jurine had recognised the true nature of these organs, but had not discovered the middle part of the brain which supports the small eye, and no more had he seen this eye itself. Mr Strauss, moreover, had not perceived that portion of the feelers which is hidden, he had only seen and delineated their projecting extremities. In many species he mistakes the optic nerve for the simple eye; and he regarded that eye as a simple point or black spot, although it presents the same organization as the eye of the Cyclops, which he himself regards as the eye, and though that organ possesses an optic nerve which is very distinct. Mr Strauss has represented that eye as nearly of the same form as in all the species of the Daphnies, while M. Ehrenberg is satisfied that, according to the different species, there is a great difference in this point; and besides, the colour of this simple eye is not black, but red, sometimes bright and sometimes dark.

Whoever will attentively examine the eyes of the Daphnia and the Cyclops, which may very well be done with a magnifier of 200 power, will find as much reason to conclude that these organs have eyes in them, as he has for considering as such the simple eyes of the Diptera. And after this he can entertain no doubt concerning the function of the red points in the Rotatoria, and other Infusores, even down to the Monades. Doubts on the point are solely the consequences of an imperfect knowledge of the connection and extent of the organs of the same name in other animals.

It is quite natural that the spot of coloured pigment should have been observed previous to the optic nerves, which are colourless and transparent; but we are not authorized to admit the real absence of these nerves, which are often invisible on account of their minuteness and their transparency, and also on account of the opacity of the parts which surround them, in the places where the coloured points exist. On the other hand, the absence of a spot of coloured pigment does not prove that the brain is awanting; for we know there exist species among the mammiferæ in which the eyes waste and almost disappear, whilst the brain does not participate in this diminution. It is indeed probable, according to all that is known, that nervous matter exists throughout the whole of the animal series.

The genus Daphnia, then, being provided with simple and compound eyee, the Cyclops with simple eyes only, and, finally, the connection of these eyes with the brain being very conspicuous, our author contends that these are proofs sufficient to dissipate all the doubts which have hitherto existed concerning the nature of the black-coloured pigments which are placed in the interior of the head of many animalcules. Add to this, that M. Ehrenberg has, in a great number of instances, very distinctly observed the knots of medullary substance communicate with the spots of red pigment in the Rotatoria, and has represented them in many, as in the subjoined figures. These are the reasons which have led him to maintain the existence of nerves in the Infusoria, reasons which are not hypothetical, but confirmed by a series of experiments.

In conclusion, it may be remarked, that the whole of M. Ehrenberg's observations have led successively to the recognition, in the Infusores, the smallest of all beings which man can perceive by those means of investigation which optical instruments supply, all the systems of organization which constitute the essential part of the organization of man; and these systems are not rudimentary, but, after their kind, as perfect as in man, although with different forms; and it may be perceived that the animal organization in man, and in the Rotatoria, and even in the Polygastric Monad, consists of a single type, which predominates throughout the whole series of animals. And moreover, every thing that he has advanced on these organized beings, so infinitely small, is not a bold and groundless philosophical speculation, but solely the result of innumerable observations, which have not yet been brought to a conclusion, notwithstanding the length of time he has already spent upon them.

IETEOROLOGICAL OBSERVATIONS made at Fort Vancouver from June 7. 1833 to May 31. 1834. By Dr M. Gairdner. Communicated by the Author.

	General Annual Means.		June.	July.	August.	September.	October.	November.	December.	January.	February.	March.	April.	May.
Barometer, nglish inches, educed to 0° C.	9 A.M.	30.065	30.099	30.065	29.940	30.048	30.076	30.163	30.069	30.099	30.122	30.023	29.980	30.103
	Noon,	30.047	30.057	30.042	29.927	30.002	30.046	30.202	30.055	30.050	30.145	30.003	29.965	30.071
	3 P.M.	30.005	29.974	29.976	29.889	30.007	30.006	30.126	30.033	30.052	30.112	29.960	29.920	30.002
	9 P.M.	30.024	not obs.	not obs.	not obs.	30.029	30.039	30.110	30.036	30.141	30.074	29.999	29.971	29.733
	Max.	30.709	30.313	30.272	30.194	30.279	30.410	30.417	30.709	30.594	30.580	30.450	30.322	30.431
	Min.	29.404	29.694	29.754	29.745	29.785	29.418	29.644	29.459	29.555	29.594	29.546	29.404	29.610
	Range,	1.305	0.619	0.518	0.349	0.494	0.992	0.773	1.250	1.039	0.986	0.904	0.918	0.821
hermometer of ahrenheit cor- ected for ind. rr.	7–8 A.M.	45°.6	58.4	59.4	58.4	54.3	45.8	39.3	36.0	23.3	36.7	39.7	44.8	51.5
	Noon,	57.4	71.5	75.5	74.5	66.7	58.6	48.3	41.6	31.1	46.1	50.1	61.6	63.5
	3 P.M.	58.6	72.5	77.5	77.7	68.8	59.7	48.3	41.0	32.2	47.8	51.5	62.8	64.0
	Sunset,	52.9	64.1	67.2	69.3	62.3	54 2	45.3	39.0	29.3	42.9	47.2	54.7	59.4
	Max.	89	83	89	89	87	73	63	52	42	63	64	81	85
	Min.	5	46	51	51	49	35	27	8	5	27	29	31	41
	Range,	84	37	38	38	38	38	36	44	37	36	35	50	44
Days of Rain.		130	7, 8, 9, 15, 16, 28 = 6	6, 7, 8, 24, 29 = 5	7, 8, 9, 10, 27, 30, 31 = 7	1, 12, 13, 14, 16, 17, 19, 21, 25 to 28 29 – 13	3, 10, 11, 12, 20, 21, 22, 25, 30, 31 – 10	1, 2, 3, 4, 5, 7, 9, 12, 17, 18, 28, 30 – 12	1, 2, 3, 4, 6, 7, 8, 10, 11, 12, 13, 15, 16, 18, 19, 20, 22, 23, – 18	8, 12, 13, 14, 31 = 5	1, 2. 3, 6, 7, 15, 16, 17, = 8	4, 5, 7 to 11, 13, 15 to 21, 25 to 28, 29, 30 – 21	4 to 7, 11, 14 to 18, 20, 21 – 12	2 to 5, 12, 13, 20 to 24, 29, 30 – 13
Depth of Rain in Eng. inches.		40.18	1.20	0.52	0.85	1.90	3.87	7.71	8.18	Pluv. frozen.	5.01	6.15	2.28	2.51
Prevailing Winds.	N. 9 S. 188 N.E. 66 SW. 139 E. 30 W. 23 SE. 306 NW. 70 Calm, 158		0 22 11 13 0 4 9 6 1	4 19 6 18 1 2 13 10 3	0 33 1 21 2 5 12 11 2	1 39 5 5 2 2 22 7 1	0 23 6 10 7 0 27 8 6	0 6 9 0 2 0 51 2 8	1 10 10 6 7 0 42 0 5	0 1 5 0 3 0 44 1 32	0 10 0 8 1 2 13 8 30	2 7 6 10 3 5 21 5 27	1 10 4 17 2 1 16 6 24	0 8 3 31 0 2 14 6 19
Days of Snow.		22	0	0	0	0	0	0	24, 26, 28, 29 = 4	7, 8, 11, 14, 15, 16, 18, 19, 25, 30 – 10	17, 18, 19, 20, 21, 22, 23 = 7	9 = 1	0	0
Days of Frost on which Therm. was below 32° F.		61	0	0	0	0	8, 9, 14 = 3	15, 16, 21, 22, 23, 24, = 6	24, 25, 26, 27, 28, 29, 30, 31 = 8	1st to 11th, 15 to 25, 28, 29, 30 – 25	11th to 15th, 17, 19th to 23d, 28 – 12	2, 3, 6, 24, 25 = 5	1, 2 = 2	0
Days of Hail.		5	0	0	0	0	0	0	23 = 1	0	21 = 1	9 = 1	7 = 1	21 = 1
Days of Thunder.		9	0	28, 29 = 2	27, 31 = 2	1, 9 = 2	0	0	0	16 = 1	0	0	5, 11 = 2	0

uver is in N. Lat. 45° 37′ 46″,4 by 17 merid. alts. of sun, and 11 merid. alts. of stars; and W. Long. 122° 33′ 38″,5 by 45 obs. of dist. sun and moon, and 29 obs. dist. moon and stars.
timated by Dalton's formula (Meteorol. Essays) from the weight collected in a close bottle, by a pluviometer of a given surface. The Columbia, which is here 1660 yards wide, and 7 fathoms
he channel, was firmly frozen over when the thermometer fell to 14° F. and continued so for upwards of three weeks.

had triumphed over the sienite of Mount Sorrel, and the limestones of Derbyshire, was broken to pieces on Flamborough Head.

Hard as is the chalk on this coast, the sea has been able to excavate many caves at the foot of the cliff of Flamborough Head. There is one which I landed to examine near what is called the north shore, being a sort of haven for fishing boats. I found it to have only a low and narrow entrance from the sea, but it was about 300 feet in length, and fifty at its greatest height, and in form that of a boat turned over with the keel upwards. As the sea is able to make caves of far greater extent on coasts formed of granite and sandstone, it is not to be wondered that it has forced its way into the chalk.

As other materials are so abundant in Yorkshire, chalk is not employed for the purpose of building in any place where I was, but there can be no doubt that it would be sufficiently durable.

A large portion of the upper part of the Yorkshire chalk is without flint. This is certainly not peculiar, for the same is the case on Dunstable Downs, in some places in Norfolk, and to a small extent at Northfleet, and in many other places. Still in the south the absence of flint on the upper chalk is the exception; but it is not so in Yorkshire. For a long way between Bridlington and Flamborough Head, no flint is at all to be seen, and in the lofty cliffs of Flamborough Head, and to the north of it, there is no flint for a hundred feet and more from the top, and it is only towards the lower part that it is to be seen, where it appears in tabular strata.

At the landing place called the north shore, the cliff is very low, only a little more than fifty feet; and the section is as follows:

3 feet covered with grass.
30 feet chalk without flint.
20 feet chalk with seven strata of tabular flint. The top stratum of flint is 6 inches thick, five strata are 2 inches each, and one stratum, the lowest, is 4 inches thick.

The chalk in the cliff, and between the cliffs and the sea, is remarkably hard.

The chalk in the cliffs of Yorkshire is more distinctly stratified than the chalk in the south. This is very perceptible all the way round Flamborough Head, and onwards to Speeton Sands, and I observed the same thing while passing some quarries near the road side between Scarborough and Bridlington. In these quarries, as well as in parts of the cliffs near Bridlington, were thin strata of earth, between the strata, which is a thing rarely seen in the south.

In many, indeed in most, of the quarries in the south of England, were it not for the horizontal lines of flint, we should not be led to form any idea of their being stratified, however our knowledge of the formation would lead us to know that it must be so. Frequently the fissures and divisions in the chalk almost obliterate the appearance of horizontal stratification, but all round the chalk cliffs in Yorkshire, the appearance of stratification is as decided as it can be, and is as much so as in the limestone in Derbyshire, or oolite in Dorsetshire, Wiltshire, or Gloucestershire. In most places the strata were about two feet in thickness. Under the Specton cliffs, for the extent of several hundred yards, the strata of the chalk towards the foot of the cliffs to the height of twenty feet, and the ledges on the beach extending down to the sea, were of a red colour. This has been attributed with good cause to iron diffused through it, and colouring it. In another place, for an extent of 500 yards, red chalk was visible at the foot of the cliff, and extending down the beach to the sea. In the Yorkshire chalk, in several places I observed extensive veins of fine calcareous spar, of about a quarter of an inch in breadth. I have never seen similar veins in any of the numberless quarries and cliffs which I have examined in the southern counties.

Under Specton there is in one place what has been called contorted strata of chalk, but I consider that the phenomenon is exceedingly simple. The appearance clearly shews, from some cause or other, that there has been a depression; and the consequence has been, that the strata have sunk down in that part forming bending lines, and some of the chalk between has been thrown forward to fill up the vacant space so produced. In Burlington Bay there is a similar appearance, only on a smaller scale.

In Yorkshire the flint differs exceedingly from the flint in chalk in the southern counties. The Yorkshire flint is found only in large tabular masses. In the southern counties, flint is found in round balls, varying in size from that of a musket bullet to that of a six-pounder ball. Sometimes flint is in the form of a tube, but by far the greater part is in nodules or tubers of every sort of form, but always bounded by curved lines, and much resembling the forms which melted lead will assume when poured into water. Horizontal lines of such flints are found in most quarries and cliffs; and in cliffs of the same height as that of Flamborough Head, as many as from seventy to eighty may be counted, but none such are seen at Flamborough Head. We find in the southern counties tabular flint, being flint in one continued mass, the extent of which is to be measured by the acre or a square mile, but it is rare to see more than one or two such lines along any cliff. But at Flamborough Head, there were strata of tabular flint and of no other description, and only towards the foot of the cliff, and five or six or seven strata, or more, at short distances, varying from one foot to five feet from each other. Some of the strata were about two inches in thickness, but some were as much as a foot. In fact, the strata of flint in the Flamborough Head chalk, bears a striking resemblance to the strata of chert, as seen in the oolite in the cliffs on the west side of the Isle of Portland. From the masses of flint which I observed amongst the heaps of chalk laid down for six miles along the road between Malton and Scarborough, it was evident that the flint there was of exactly the same description as at Flamborough Head. As a qualification to the preceding remarks, it ought to be stated, that some few small round flints, the matter of which had probably aggregated about some animal substance, were to be seen on the beach.

As to the colour of the Yorkshire flint, instead of deep black, which is usual in the south of England, in flints found in chalk, it was of greyish or whitish colour, but not uniform; and some parts were much whiter than others. Though distinctly different in colour from the bright whiteness of the chalk, it is not so much so as to render the flint strata visible or conspicuous, unless at a short distance.

When the hammer is applied to the Yorkshire flint, the difference between it and the flint in the south of England is found to be very great. It is not easily broken into long flakes. It cannot be broken with ease in any direction. On the contrary, the Yorkshire flint is a most unmanageable substance, not easily breaking without great violence, and then the fragments are short, small, and clumsy, and not yielding a graceful form and long sharp edge. It is totally unfit for the gun-flint maker. It can never have supplied knife-blades and kelts to the ancient Britons, and innumerable fragments must have been struck off before any suitable for a spear-head, or the point of an arrow, could have been procured. In a barrow opened near Scarborough, the head of a spear, and the points of arrows, were found along with the skeleton of a chief, and these are to be seen in the Scarborough Museum, and have been described and figured in a publication by Mr Williamson junior. They are as clumsy as it is possible to conceive, and far inferior to what might readily, by the most inexperienced, be struck from the flint found in the south.

The outside of the Yorkshire flint does not, by exposure to the action of the air and water, acquire the thick white crust which is uniformly produced under similar circumstances, in the flint from chalk in the southern counties, which is often supposed, from its appearance, to be a combination of flint and chalk, but which is seldom any thing else but the flint itself. The Yorkshire flint, on exposure, becomes on the outside of a reddish-brown colour, not unlike rust from iron ; but, on the application of the hammer, it is found to be merely on the very superficies scarcely the thickness of a coat of paint, and below it the flint is unaltered.

Nodules of pyrites were frequent, which is sometimes, but not often, the case in the south, until we come to the chalk below the flint.

Altogether, the flint at Flamborough Head bears a great resemblance to the chert brought from Flintshire to the potteries, in different parts of England, and which is employed for the purpose of grinding the flint found in chalk, after it has been rendered white and friable by being burnt in the fire.

From observations on the chalk formation in Ireland by Mr Conybeare and Dr Buckland, published in the Geological Transactions, vol. iii. p. 169, 170, it would appear that the chalk of Yorkshire and the chalk of Ireland were very much alike. The chalk of Ireland is stated to be so hard, that it is known by the name of White Limestone. It is traversed by slender veins of calcareous spar, which are more frequent in the lower beds. It contains, though in very small quantities, kidney-shaped nodules of iron-pyrites. The most striking of its imbedded contents are said to be flinty nodules, which traverse the mass in regular horizontal strata.

The chalk in Ireland is stated in that paper to have some other peculiarities; but the above are exactly applicable to the chalk of Yorkshire.

On the Infra-orbital Cavities in Deers and Antelopes, called Larmiers by the older French Naturalists. By ARTHUR JACOB, M. D., Professor of Anatomy in the Royal College of Surgeons in Ireland.*

In compliance with the recommendation of the Committee of the Zoological Section of the Association made at the meeting in Cambridge in 1833, I have availed myself of such opportunities as have been afforded me of investigating the nature, structure, and uses of these remarkable parts. To those altogether unacquainted with the subject it is necessary to state that they consist of two oval depressions about an inch and half long, half an inch wide, and more than three quarters of an inch deep in the majority of instances; situated on the side of the face, and so near to the inner angle of the eye that they create a very reasonable suspicion that they are connected with that organ, and hence the term *larmier* applied to them. The bottom of the depression is in most cases naked, but in some it is covered with the hair, consequently it is composed of the skin formed into an open sac, accommodated in a corresponding depression in the bones of the face. In many animals provided with this organ, a gutter, formed by folds of skin, leads so directly to it from the surface of the eye, that the passage of the tears from the one place to the other appears inevitable; while

* Read at the Meeting of the British Association held in Dublin, August

in others this communication is so imperfect that a doubt is at once raised as to its destination to such a purpose. If the part in question be not a cavity, as suggested by some, in which the overflowing secretions from the surface of the eye are disposed of by evaporation, another reason for its existence must be assigned. The arguments which may be urged against the supposition that it is destined to receive the tears are, first, that it exists in the antelopes and deers only, and is even absent, or merely rudimental, in many of these; while in animals said to be destitute of the usual canals for carrying off the tears to the nose, as the elephant and hippopotamus, it is absent; secondly, that the solid concretions generally found in it are not composed of such ingredients as the tears and other secretions from the surface of the eye should afford.

If the conclusion that these are cavities for the reception of tears be discarded, their identity of nature and character with the numerous provisions for the secretion of peculiar or odoriferous materials suggests itself. In many instances, especially in the mammalia, glands are found opening on the surface of the skin, and pouring out peculiar fluids, sometimes altogether unconnected with any organ; such are the glands on the side of the head between the eye and ear of the elephant, those described by Tiedemann between the eye and nose in certain bats, consisting of a sac with a folded lining membrane, affording a fœtid, oily secretion, and beneath the eye in the marmot and two-toed ant-eater; such also are the glands on the side of the chest of the shrew, described by St Hilaire, and the inguinal glands of hares. Still more remarkable examples are furnished by the pouches, affording the valuable odoriferous materials in the musk, beaver, and civet; and if additional examples be required, they are found in the otter, male hyena, ichneumon, badger, and the dorsal gland in the peccary. That the cavities alluded to in the deers and antelopes afford peculiar and often odoriferous secretions, is established on the authority of several naturalists. Buffon describes the contents in the stag as resembling ear-wax. Daubenton found the secretion in an old stag so much indurated as to constitute a solid mass, or bezoard, as he calls it, eleven lines long, seven broad, and six thick. Camper found hard, yellowish particles in the fallow deer. In a species of antelope first described by Dr Herman Grimm, this organ secretes a fluid of such peculiar and distinct character that no doubt can be entertained of its nature. He describes it to be a yellowish, fatty, and viscous humour, having an odour between musk and camphor. Vosmaer says that it hardens and becomes black in time, and that the animal rubs it off on the rails of its cage, but he could not detect the musky odour. Pallas, who describes the Antilope grimmea particularly, concurs in these observations.

It may be objected to the conclusion, that these are organs for the production of an odoriferous secretion, that the sac exhibits so little of glandular character that it appears inadequate for the purpose, especially when several of the external openings alluded to, as that on the head of the elephant and the back of the peccary, are merely the outlets of considerable glands; but on the other hand, many organs of this character are mere sacs, as that on the face of the bats, the bottom of which presents a peculiar folded appearance, and the cavities in the musk and beaver, which afford the odoriferous secretion in such large quantity.

A statement respecting these infra-orbital cavities has been made by the Rev. Gilbert White in his Natural History of Selbourne, which might appear to originate in some error, were it not supported by the more recent testimony of Major Hamilton Smith. These gentlemen state, that when the deer drinks, the air is forced out through these cavities, and, according to Major Hamilton Smith, may be felt by the hand, and affects the flame of a candle when held to it. Notwithstanding such a positive statement by two observers of established character for faithful description, the passage of air through these cavities cannot take place, they are perfectly impervious toward the nostril; but I have no doubt that the fact stated is correct, the air which escapes passes, not through the infra-orbital sacs, but through the lachrymal passages, which are very large, consisting of two openings capable of admitting the end of a crow's quill, the entrance to a tortuous canal, which conducts the tears to the extremity of the nose. Introducing a pipe into the outlet of the nasal duct at the extremity of the nose, I can, without difficulty, force a current of air or water through the nasal duct; and it therefore appears reasonable to admit that the effect observed by the two gentlemen alluded to, arose from the animal forcing the air into the nostrils while the nose and mouth were immersed in water. Even in the human subject air may be forced up the nasal duct into the lachrymal sac, by filling the cavities of the nose from the lungs while the nostrils are closed by the hand.

Persons following up this investigation should be aware that these cavities exist in a very imperfect state in many species, being, in fact, merely rudimental, and incapable of affording the secretion which they are destined to provide in others. The last traces of the organ may even be detected in goats, sheep, and perhaps all the ruminants. It is a beautiful example of that adherence to an original type or model which is so conspicuous in animal organization, and as if in obedience to a law that all the ruminants should be provided with a sinus beneath the eye for the secretion of a peculiar odoriferous matter, but that it should remain in an imperfect or unfinished state in those who do not require such additional aid to distinguish sex or recognise species.*

Since the above was written I have had an opportunity of examining these sinuses in the wapiti (Cervus canadensis), and obtained from one of the cavities a large solid mass of the indurated secretion like that found in the sinuses of the stag by Daubenton, and called by him *bezoard de cerf.* This, Dr Geoghegan, the Professor of Medical Jurisprudence in the Royal College of Surgeons, has been kind enough to submit to analysis, the results of which corroborate the inference that the secretion found in the cavities is derived from the cavity itself, and not from the surface of the eye. The existence of the hairs and flakes of exfoliated cuticle in layers proves that the deposit is formed from the surface beneath, and not by evaporation of fluids trickling into the cavity. Dr Geoghegan's account of the analysis I annex in his own words.

" The bezoard described by Dr Jacob is covered by a fine transparent membrane, a good deal resembling goldbeater's leaf; within this, and arranged concentrically, are four or five laminæ, having a coriaceous appearance; these seem to be soaked with the dark brown matter which constitutes the great bulk of the mass. The thickness of these membranous coverings is altogether about a line and half. The matter contained within this covering is of a dark reddish-brown colour, resembling indurated cerumen, and consisting apparently of a number of fine hairs matted together by a substance of an oleo-resinous appearance. This substance in one specimen was viscid and tenacious, and of the consistence of common turpentine; while in another it was more friable. Both exhaled a most peculiar odour resembling soft soap made with fish oil, but slightly pungent and aromatic. The more friable specimen had the smell of kreosote when much diluted. The specific gravity of the large mass was 1.081. The material has a slightly bitter taste, but does not dissolve in the mouth, and imparts a very slight greasy stain to paper. When heated it swells, grows darker in colour, and undergoes a partial fusion; and if the heat be increased it takes fire, and burns with a bright flame and much smoke, leaving behind a greyish-white ash. A fragment digested with five successive portions of water, imparted to them the peculiar odour of the substance, which was, however, dissipated by evaporation. It appears therefore to contain a volatile odorous principle, which is so intimately combined with the other principles present, that even after digestion in the above-mentioned number of waters, the residuum, which was but little acted on, possessed its peculiar odour nearly as strongly as before. The aqueous solution afforded on evaporation a brownish extractive matter, with which nitrate of silver gave a copious precipitate of chloride of silver; and oxalate of ammonia indicated a salt of lime, most probably lactate. Another portion digested in æther coloured it yellow, and the solution on evaporation furnished a yellowish-brown transparent substance, very viscid and tenacious at ordinary temperatures, very readily fusible, and exceedingly soluble in caustic potash; immediately on uniting with which, it exhales strongly the smell of fish-oil soap. This solution is miscible with water without decomposition; acids precipitate a white matter, and when, subsequent to the addition of acid, the mixture is heated, an oily looking matter floats, and the rest of the fluid becomes turbid and milky. Cold alcohol digested on another portion took up a good deal of yellow viscid matter; and when evaporated furnished also some extractive, soluble in water, probably the same as that afforded by the aqueous solutions. Boiling alcohol, digested on the residuum, takes up more of the yellow matter, which, on evaporation, affords a more resinous looking residuum, the surface of which is covered with a greasy film, also saponifiable by caustic potash. Alcohol, digested on what remained after the action of æther, dissolved only a trace of saline matter; and the residuum, after exhaustion by æther, had the appearance of thin flakes of pearly cuticle, coloured yellowish-brown, insoluble in strong acetic acid, but soluble in potash, from which it was precipitated by acetic acid. A portion of these flakes, when strongly heated, left a white ash, consisting of carbonate and phosphate of lime, carbonate of soda, and chloride of sodium. The material then appears to consist of a number of hairs, with a quantity of delicate, cuticular flakes, the whole intimately mixed with a dark matter, composed as follows: a brownish, viscid, oily substance, probably containing resin; a volatile odorous principle; extractive, soluble in water and alcohol; colouring matter, which adheres to the flakes of cuticle; lactates of soda and lime, a trace of phosphate of lime, and chloride of sodium in considerable quantity."

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* The authorities quoted are Buffon in the original quarto edition, t. vi, and Suppl. t. iii.; Pallas, Spicilegia Zoologica; White's Natural History of Selbourne: the supplementary volume of Griffith's Translation of Cuvier on the Ruminants, by Major Charles Hamilton Smith: and Camper, Œuvres, t. i.

Remarks on the Difficulty of distinguishing certain Genera of Testaceous Mollusca by their Shells alone, and on the Anomalies in regard to Habitation observed in certain Species.

By John Edward Gray, Esq. F.R.S. &c.*

It has been a very common error, both among conchologists and geologists, to regard all shells in which no remarkable difference of form and character can be distinguished as inhabited by one and the same genus of animals; and not less usual to assume that all the species of the same genus inhabit similar localities. Many geologists have still further enlarged the boundaries of error, by taking for granted that all the fossil species of shells which are referable by the characters of the shell to recent genera, must have been formed by animals which, in their recent state, possessed the same habits as the most commonly observed species of the genus to which they appear to belong. These theories were, indeed, quite consistent with our former ignorance of the habits of the animals of this class; but since the works of Poli, Müller, Montagu, Lamarck, and Cuvier have induced zoologists again to turn their attention, as was the practice among the older writers, to the animals of shells, and their habits, and no longer to confine themselves, as was too often the case with the followers of the Linnean system of conchology, to the study of the shells as mere pieces of ornament, classed without reference to their inhabitants, the acknowledged importance of the subject is daily bringing to our knowledge some animal unknown before, and adding to our stock of information facts which prove the fallacy of the opinions so hastily taken up. Thus, although even at the present day the animals of less than one-twentieth part of the well-known species of shells have been observed—and of those which are known the greater part have been very imperfectly described—numerous exceptions to the theories in question have been brought to light, which deserve to be collected into one point of view, and made the subject of serious consideration.

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* This valuable memoir (from the Philosophical Transactions, Part II. for 1835) so interesting to the zoologist, contains also statements of great importance to the practical and speculative geologist. We, therefore, lay it before our readers without abridgment or alteration.—Edit.

The exceptions which it is the object of the present paper to notice, may be arranged under the two following heads:—

1. Shells having every appearance of belonging to the same natural genus, but inhabited by animals of a very different character.

2. Species of testaceous *Mollusca* living in very different situations from the majority of the known species of the genus to which they belong, or having the faculty of maintaining their existence in several different situations.

These two classes of exceptions I shall proceed to notice in detail.

1. *On Shells apparently similar, but belonging, on a comparison of their Animals, to very different Genera.*

In a note on my former paper on the structure of shells,* I pointed out the perplexity in which the extreme similarity of the shells belonging to the genera *Patella* and *Lottia* must involve the geologist and the conchologist, intending at some future time to pursue the subject further, and to shew that similar difficulties existed in regard to several other genera. The two genera above referred to are probably, however, the most remarkable example of this complete resemblance, on account of the extreme dissimilarity of their animals, which are referable to two very different orders of *Mollusca*, while the shells are so perfectly alike, that after a long-continued study of numerous species of each genus, I cannot find any character by which they can be distinguished with any degree of certainty. Both genera present a striking discrepancy from all other univalve shells, in having the apex of the shell turned towards the head of the animal, the genera to which they are immediately related in both the orders to which they belong, offering no variation in this respect from the usual structure of the class. The agreement in the internal structure of their shells is equally complete; yet the animal of *Patella* has the branchiæ in the form of a series of small plates disposed in a circle round the inner edge of the mantle, while that of *Lottia* has a triangular pectinated gill seated in a proper cavity formed over the back of the neck within the mantle, agreeing in this respect with the inhabitants of the *Trochi*, *Monodontæ*, and *Turbines*, from which it differs so remarkably in the simple conical form of its shell. This difference in the respiratory organs of animals inhabiting shells so strikingly similar is the more anomalous, inasmuch as those organs commonly exercise great influence on the general form of shells; a circumstance readily accounted for when we reflect that a principal object of the shell is to afford protection to those delicate and highly important parts.

To the practical conchologist it will be sufficient to mention *Pupa* and *Vertigo*, *Vitrina* and *Nanina*, *Rissoa* and *Truncatella*, as affording numerous and perplexing instances of the difficulty of distinguishing between genera of shells, inhabited by very different animals.

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* Philosophical Transactions, 1834, p. 800.

A similar difficulty exists with regard to *Siphonaria* and *Ancylus*, genera belonging to different families, one inhabiting the sea-shores, while the other lives in rivers and brooks. The only distinction between the shells of these two genera consists in the *Ancyli* being generally of a thinner substance than the *Siphonariæ;* but this is by no means an adequate character, some species of *Siphonaria* (*S. Tristensis*, for example), being quite as thin in texture as any *Ancylus.* Both have the muscular impression interrupted by the canal through which the air passes to the respiratory organs; yet the animal of *Ancylus* has long tentacles, and eyes placed as in the *Lymnææ*, to which it is closely allied, while *Siphonaria* has no distinct tentacles, and in these respects agrees with the equally marine genus *Amphibola*, confounded by Lamarck with the *Ampullariæ.*

About fifteen years since, I first observed, in the marshes near the banks of the Thames, between Greenwich and Woolwich, in company with species of *Valvata*, *Bithynia*, and *Pisidium*, a small univalve shell, agreeing with the smaller species of the littoral genus *Littorina* in every character both of shell and operculum; yet this very peculiar and apparently local species has an animal which at once distinguishes it from the animal of that genus, and from all other Ctenobranchous *Mollusca.* Its tentacles are very short and thick, and have the eyes placed at their tips; while the *Littorinæ*, and all the other animals of the order to which they belong, have their eyes placed on small tubercles on the outer side of the base of the tentacles, which are generally more or less elongated. The shell in question and its animal were described and figured by Dr Leach, in his hitherto unpublished work on British *Mollusca*, under the name of *Assiminia Grayana;* and as this name has been referred to by Mr Jeffries and other conchologists, it may be regarded as established, and that of *Syncera hepatica*, proposed by myself in the Medical Repository, vol. x. p. 239, will take the rank of a synonym. A second species of this genus has lately been made known by Mr Benson, by whom it was found in ponds in India. Its shell is banded like that of *Littorina* 4-*fasciata* and several others of the smaller *Littorinæ*, and had been figured in the

Supplement to Wood's Catalogue, t. 6. f. 28, under the name of *Turbo Francesiæ.*

Taking this in conjunction with the preceding, we have here two instances of univalve shells, apparently belonging to the same genus, the one found in fresh and the other in salt water, but proving, when their animals are examined, to belong to genera essentially distinct. My next illustration will shew that a similar fact has been observed among the bivalves.

The *Mytilus polymorphus* of Chemintz is truly a fresh-water species, having been first observed in the Wolga by the illustrious Pallas. It has recently been introduced, doubtless with the Russian timber (for this species, in common with the *Ampullariæ*, *Paludinæ*, and *Neritinæ* of fresh water, and the *Littorinæ*, *Monodontæ*, and *Cerithia* of salt, has the faculty of living for a very long time out of water), into the Lake of Haarlem and the Commercial Docks at Rotherhithe; in both of which it appears to increase with great rapidity. I am aware that Mr Lyell has given another explanation of the mode of introduction of this remarkable species; but from experiments which I have myself made on the animal's power of living out of water, I cannot hesitate in giving the preference to the suggestion advanced above, rather than supposing it to have made its passage from one river to the other, across the sea, attached to the bottom of a vessel. The shell in question differs from the shells of other *Mytili* in no character of more than specific importance; but the animal is essentially distinct. In the genus *Mytilus* the lobes of the mantle are free throughout nearly their whole circumference, as in *Unio*, *Cardita*, *Pecten*, *Ostrea*, &c.; while in the animal of *Mytilus polymorphus* they are united through nearly their whole extent, leaving only three small apertures, one for the passage of the foot and beard, and the other two for the reception and ejection of the water, from the contents of which the animal derives its sustenance. This shell must consequently form a new genus, to which the name of *Dreissena* has been appropriated by Van Beneden.* As a proof of the importance attached to this character, it may be observed that Cuvier considered the adherence or non-adherence of the lobes of the mantle so essential a distinction as to found on it his division of the bivalves into families. In his system, therefore, the genus *Dreissena* would be placed with the family of *Chamacées,* while the genus *Mytilus* forms the type of the preceding family of *Mytilacées.* The genus *Iridina,* however, and one or two others, shew that this character cannot be implicitly relied on for the natural classification of animals of this class, although it forms a very good generic mark of distinction.

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* Institut. 1835, p. 130; and Annales des Sciences Naturelles, S. N. tom. iii. p. 193.

The genus *Iridina** above referred to affords a second instance of this anomaly; for though the animals of the *Iridinæ* and *Anodontæ* differ in the adhesion and non-adhesion of the lobes of the mantles, yet the shells are so alike that they cannot be distinguished by any external character; so much so, that one of the species now referred to the genus by M. Deshayes, who first pointed out this peculiarity in the animal, was considered as an *Anodon* by Lamarck.

The animals of *Cytherea, Venus,* and *Venerupis* have, like those of most of the allied genera, a lanceolate foot projecting at the anterior part of the shell; while the genus *Artemis* of Poli, which has generally been confounded with *Cytherea,* from which it is not easily to be distinguished except by its usually more rounded form, is provided with a crescent-shaped foot, exserted at the middle of the lower edges of the valves.

Again, there is but little difference in external characters and habit between *Cyclas and Pisidium;* but the animals of the latter have elongated syphons, which are not found in the former.

In reference to Univalves it may also be observed, that it is frequently impossible to distinguish some of the genera of that class without an examination of their opercula. This is the case, for instance, as regards the smaller and more solid *Paludinæ,* inhabitants of fresh water, and some species of *Littorina* living on the coast; several of the shells described, as *Paludinæ* by Draparnauld and others, appearing rather to belong to the latter genus.

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* Lamarck formed this genus on a specimen which had its hinge margin accidentally tubercular and slightly crenated; but this character is not found in most of the specimens of the species which he describes. The English conchologists, misled by this character, have referred to the genus a very different African shell, with a long series of transverse teeth on the hinge margin, which has lately been separated by Mr Conrad under the name of *Pleio-*

A similar difficulty exists with respect to other *Littorinæ* as distinguished from *Phasianella*, and with the *Neritinæ* as distinguished from the *Neritæ*. In the latter case the characters derived from the operculum are so essential to the discrimination of the two genera, that M. Rang, looking only to the characters of the shell, has proposed to reunite them into one. In proof of the little attention that has hitherto been paid to this very important part, I may mention that three species referred by Lamarck to the genus *Solarium* are each furnished with a different kind of operculum; and it is deserving of notice that the *Monodonta canaliculata* according to the observations of M. Quoy, has an operculum very different from the rest of the shells of that genus.

In some shells, again, the differences in character are so slight as almost to throw an air of ridicule on the attempt to separate them generically from the structure of the shells alone; and yet, when the animal is examined, the necessity of their separation becomes so obvious as to be immediately acknowledged. This is especially the case with my genus *Bullia* compared with *Terebra:* the shells of these two genera are so similar, that Lamarck and all other conchologists have retained them in one group, no other distinction being observable except that in the former there is a more or less distinct callous band winding round the volutions just above the suture, and produced by a slight extension of the inner lip beyond the part of the shell occupied by the whorl. This extension of the lip is probably deposited by the foot of the animal, which in the genus *Bullia* is very large and expanded, while that of *Terebra* is small and compressed. This, however, is not the only difference between the two animals, that of the former genus having rather large and eyeless tentacles, while the *Terebræ* have very small and short tentacles, bearing the eyes near their tips.

A second example of a similar kind is derived from the genus *Rostellaria*, in which Lamarck includes the *Strombus Pes Pelicani* of Linnæus. The animal of this shell has been figured by Müller, and very much resembles that of *Buccinum*, having long slender tentacles with the eyes sessile on the outer side of their base; while, as Dr Rüppell informs me, the *Rostellaria curvirostris* has an animal allied to *Strombus*, with the eyes on very large peduncles, which give off from the middle of one of their sides the small tentacles. Notwithstanding this difference in the form of their animals, I am not, however, aware of any essential character by which the shell of *Aporrhais* (as the *Strombus Pes Pelecani* has been generically named) can be distinguished from the other *Rostellariæ.*

With all this uncertainty with regard to the generic characters of the recent species of shells, of which the animals can be subjected to examination, how much must the difficulty of deciding their genera with certainty be enhanced with reference to the fossil species, and especially to those which have no strictly analogous form existing in the recent state. Considerations like these tend greatly to disturb the confidence formerly reposed in the opinion, that every difference in the form and structure of the animal was accompanied by marks permanently traced upon the shell, by which it might be at once distinguished, and which it was therefore the great object of the conchologist to point out. But another source of error, particularly interesting to the geologist, is included under my second head, to the elucidation of which I shall now proceed.

2. *Of Species belonging to the same natural Genus, inhabiting essentially different situations.*

The general belief that all the species of the same genus inhabit the same kind of situation, undoubtedly holds good with reference to most of the genera of shells; but many exceptions have already been observed, and we may anticipate that many more will be discovered as the natural habits of the different species become better known. In bringing together a number of these exceptions, I have been under the necessity of placing considerable reliance on the observations of others, who have noted in foreign countries facts similar to those which I have myself witnessed at home; but these observations have been chiefly collected from the works of Professor Nilsson of Sweden, of Mr Say of the United States of North America, and of MM. Lesson, Quoy, and Rang of Paris, writers who, from their extensive knowledge of conchology, are fully capable of accurately recording their observations, and whose statements may therefore be received as deserving of the most implicit confidence. It is moreover to be observed, that all their observations on this subject were made simply with the view of extending the knowledge of the history of the species to which they refer, and without reference to the establishment of any preconceived theory.

These observations may be classed under the four following subdivisions;—1st, where species of the same genus are found in more than one kind of situation, as on land, in fresh and in salt water; 2d, where one or more species of a genus, most of whose species inhabit fresh water, are found in salt or brackish water; 3d, where, on the contrary, one or more species of a genus, whose species generally inhabit the sea, are found in fresh water; and 4th, where the same species is found both in salt and fresh water.

Of the first of these classes the genus *Auricula*, as defined by Lamarck, may be quoted as a striking example. Of its species, *A. Scarabus* and *A. minima* are found in damp places on the surface of the earth; *A. Judæ* lives in sandy places overflowed by the sea; *A. Myosotis*, *A. coniformis*, *A. nitens*, &c. (separated by De Montfort under the name of *Conovulus*), are found only in the sea in company with Chitons, *Littorinæ*, and other truly marine shells; and the South American species which I distinguished some time since under the name of *Chilina*, including *A. Dombeyi* of Lamarck, and *A. fluviatilis* of Lesson, inhabit fresh-water streams, having most of the habits of the *Lymnææ*. This disparity of habitation has been in some degree overcome by dividing the genus into several, as noticed above; but the characters employed for their distinction are very slight, and species apparently intermediate between them are constantly occurring.

The genus *Lymnæa* has usually been considered as confined to fresh water; but M. Nilsson describes a species under the name of *L. Balthica*, which is found "in aquâ parùm salsâ Maris Balthici ad littora Gothlandiæ et Scaniæ, &c. In maris juxta Esperöd fucis et lapidibus adhærens frequenter obvenit simul cum *Paludinâ Balthicâ* et *Neritinâ fluviatili*;" and a second under the name of *Lymnæa succinea*, which is found on the shores of the sea near Trelleborg. All the species of *Paludina* and *Bithynia* which have fallen under my own observation are essentially fluviatile; but M. Nilsson refers in the paragraph above quoted to a species of the former genus inhabiting the sea. This may, however, like some of the smaller *Paludinæ* of Draparnauld, be truly a *Littorina*, having a horny and spiral, and not an annular, operculum.

According to the observations of my sister, Mrs Ince, of Mr Benson, of MM. Quoy and Gaimard, and of M. Lesson, the Indian species of *Neritina* like the European, are found only in fresh water; yet M. Rang, in his Manuel des Mollusques, p. 198, states that the *Neritina viridis* is a marine species found on rocks covered by the sea at Martinique, and that a larger variety of this species is found in similar situations at Madagascar; General Hardwicke marks on his drawing of the *Neritina crepidularis*, that it was found in "saltwater lakes, April 1816;" and Say has described the *Neritina Meleagris* of Lamarck (*Theodoxus reclinatus*, Say) as living both in fresh and salt water. This is most probably the species to which Mr Guilding refers,* when he observes that he has kept *Neritina* for some time alive in a close vessel of salt water, which they appear to purify. The animals of some of the tropical species often quit the stream and crawl up the trunks of neighbouring trees, on which, like the species of *Littorina*, *Planaxis*, and *Bulla*, which creep up the rocks on the sea-coast, they attach themselves, and remain exposed to the influence of the sun. It may be added, that M. Rang has found *Neritina Auricula* in brackish marshes near the sea in the Island of Bourbon, in company with *Aviculæ* and *Aplysiæ;* and I have little doubt that *Neritina Pupa* inhabits the sea, it being uniformly brought to this country in company with marine shells.

Many species of *Melania*, as, for example, *M. amarula*, *M. fasciolata*, and *M. lineata*, are found in the fresh water streams of India and its islands. Mr Say mentions species found in similar situations in North America; he also describes one (*M. simplex*) as found in a stream running through the salt water valley near the salt-works, but does not state whether the water of the stream is salt or fresh. On the other hand, M. Quoy asserts that they are sometimes taken in brackish water; M. Cailliaud states that *Melania Oweni* is found in brackish water; and M. Rang has found other species in the Island of Bourbon, under the same circumstances with the *Neritina* just adverted to. The genus *Melanopsis* has the same habits; its species are often found in large inland lakes. I have myself received *M. buccinoidea* from the sea of Galilee; and Dr Clark, in his Travels, vol. ii. p. 243, figures *M. Dufourii* under the name of *Buccinum Galileum.* The water of this lake, however, unlike that of the neighbouring Dead Sea, is, according to the statement of Fuller, perfectly fresh and sweet. M. Lesson, on the other hand, states that he found the *Pyrena terebrans*, regarded by M. de Férussac as a *Melanopsis*, in great abundance in brackish marshes in New Guinea, and at the Island of Bourou.

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* See Zoological Journal, vol. v. p. 33.

I am informed by Mr Sowerby that some species of the fluviatile genus *Cyrena*, are found in the sea on the coast of South America, but he thinks it probable that the part of the sea in which they are met with may be fresh, like certain parts of the ocean described by Dr Abel in his Voyage to China. It would be highly interesting to procure a verification of this observation. Similar phenomena may not be uncommon, for I have myself observed in Torbay a small space in the neighbourhood of Brixham, the water of which was of a different colour, and much fresher than that of other parts of the bay. With reference to another species of the same genus, *Cyrena Vanikorensis*, M. Quoy observes:—"Ne l'ayant pas trouvée dans les lieux marécageux mais sur les bords de la mer, il est probable qu'elle vit à l'embouchûre des rivières qui sont saumâtres à marée haute."*

The third class of cases, in which species of *Mollusca* that are generally found in the sea are taken in fresh water, is much more rare than the preceding. It is obvious that in such instances the animal must be possessed of the capability of adapting itself to the different characters of the two fluids. This capability exists in much more highly organized animals, such as fishes, many species of which constantly migrate from the sea and ascend the rivers to deposit their spawn; but in these cases it is the result of a regular and determinate habit, while in the *Mollusca* it appears to be entirely dependent on accidental circumstances.

In some marshes in the Island of Bourbon, in which the water is almost fresh, M. Rang has observed specimens of *Aplysia dolabrifera* in company with *Neritinæ* and *Melaniæ.*

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* Voyage de l'Astrolabe, tom. iii. p. 516.

The greater number of species of the genus *Cerithium* are truly marine, chiefly living in sandy bays, like our own *Cerithium reticulatum.* M. Lesson, however, found *C. sulcatum*, and Adanson the African species figured by him, in the pools of brackish water, sometimes overflowed by the sea, which are situated between the weeds and the belts of mangrove trees on the shore; and Mr Say observes that the small species called by him *Pyrena scalariformis*, but which is a true *Cerithium*, is found in great abundance in the fresh water of Florida Keys. He adds, " It is most certainly a fresh water shell, yet it is destitute of an epidermis."

The genus *Bulla* is also truly marine; but the Rev. Mr Hennah some time since presented to the British Museum specimens of one of its species, resembling the *Bulla Hydatis*, found by him in brackish pools on the coast of Chili; and Mr Say has described a *Bulla fluviatilis* found by Mr Aaron Stone deeply imbedded in the mud of the river Delaware.*

The *Littorinæ*, again, are all found either on the sea-shore or in the very brackish waters of the mouths of rivers, except two, which, although described as *Paludinæ* by Pfeiffer and De Férussac, and formed into a distinct genus by Ziegler under the name of *Lithoglyphus*, agree with *Littorina* in every character of shell and operculum, and, as far as I can ascertain from the descriptions, of the animal also. These are the *Paludina fusca* of Pfeiffer, and the *P. naticoides* of De Férussac; they are truly fluviatile.

These anomalies are not restricted to the univalves: bivalves have also their share. Thus, the genus *Solen* is generally and properly considered as marine; but Mr Benson has lately discovered a species inhabiting the mud on the banks of the Gánges; and conceiving, from the nature of its habitation, that it ought to be separated from the common species, he has formed a genus for its reception, under the name of *Novaculina.* On comparing, however, some specimens of the shell presented to the British Museum by Mr Royle, I can scarcely distinguish it as a species from the *Solen Dombeyi* of Lamarck, which is found on the coast of Peru; and I have two other species, very nearly related, one from the rivers of China, and the other from pools of brackish water on the coast of America. In like manner M. Nilsson has found his *Tellina Balthica*, which appears to be little more than a variety of the *Tellina solidula* of our coast, in the brackish waters of the shores of the Baltic. *Avicula margaritifera*, the mother-of-pearl shell, commonly found in the ocean, has been taken by M. Rang in marshes in the Isle of Bourbon in the neighbourhood of the sea, in which the water is nearly fresh. Specimens of *Mya arenaria* also, are often found so high up the rivers that the water in which they live is brackish only during high tides. They are found, moreover, with fresh-water shells on the coasts of the Baltic, while all the other species of the genus are found only where the water is quite salt.

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* See for this latter instance the Journal of the Academy of Natural Sciences of Philadelphia, vol. ii. p. 179.

By far the greater part of the species of *Corbulæ* are truly marine; but there is a large species of the genus, called by Dr Maton * *Mya labiata*, brought with fresh-water shells from the mouth of the Rio de la Plata; and this agrees in many respects with the fossil *Corbula Gallica*, which occurs in what are called the upper freshwater strata of the Isle of Wight.

The transitions to which the oysters intended for the London market are exposed, may be mentioned as an additional illustration. Many of these are collected in the sea on the coasts of Guernsey and of France, and are brought to situations in the mouth of the river where the water is merely brackish during the ebb of the tide, and where they are consequently subjected to the alternate action of salt and brackish water twice in each day. It is even affirmed that oysters can exist in water absolutely fresh; for in the museum of the Bristol Institution, there is a large group said to have been dredged up in a river on the coast of Africa, where the stream was so sweet as to have been used to water the ship. To these shells are attached specimens of *Cerithium armatum*; and the person by whom they were presented to the collection, stated that *Cardium ringens* was found abundantly in the same situation.

The genus *Cucullæa*, again, is universally considered as truly marine; but Mr Benson has found in the Ganges a small shell belonging to it, regarded by him as an *Arca*, but, on account of its fresh-water origin, formed into a new genus under the name of *Scaphula*.

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* Linnean Transactions, vol. x. p. 326, t. 24, f. 3.

On this subject I may observe, that I was some time ago informed that *Arca senilis* was found in the rivers of Africa in company with *Galatea radiata*: M. Cailliaud, however, assures me that this is by no means the case, the shells in question being found near the mouths of the rivers, but never in the rivers themselves.

One of the most decisive facts regarding the finding of the same species of shell in both salt and fresh water is noticed by Say.* Speaking of *Theodoxus reclinatus*, he observes, "I found this species in great plenty, inhabiting St John's River in East Florida, from its mouth to Fort Picolata, a distance of one hundred miles, where the water is potable. It seemed to exist equally well where the water was as salt as that of the ocean, and where the intermixture of that condiment could not be detected by the taste." The shell in question is determined by specimens which I received from my late friend himself (to whom science is so deeply indebted, and especially for his researches into the zoology of North America), to be the *Neritina Meleagris*, obtained in such abundance from the West Indian Islands. Nilsson, too, as before mentioned, has noticed the *Neritina fluviatilis*, which in this country is not observed to inhabit ditches in the neighbourhood even of brackish water, living on the coasts of the Baltic, in brackish situations, in company with *Lymnæa Balthica* and *L. succinea*; and M. Rang found *Neritina auriculata* in similar situations.

According to the observations of Olivier, the *Ampullaria ovata* inhabits Lake Mareotis, where it is taken in company with marine shells found also in the Mediterranean; and I have lately received (dead) specimens from the locality indicated. The same species was found by M. Cailliaud in fresh-water lakes in the Oasis of Siwah, where it is called bozue, and eaten as food. It thus appears to be found both in fresh and brackish water. Two of the species referred to this genus by Lamarck, his *Ampullaria Avellana* and *A. fragilis*, are truly marine; but they differ from the others in animal and operculum, as well as in the sinuated form of the outer lip of their shell.

The common cockle of the shops, *Cardium edule*, is constantly to be seen in the ditches of brackish water in the neighbourhood of Tilbury Fort, which gradually become more or less fresh in proportion to the quantity of rain that falls between the periods of opening the sluices. It is to be observed that the specimens found in this situation are rather thinner and more produced posteriorly than those usually found in the sea. The species in question is also, according to Nilsson, found in the brackish water on the shores of the Baltic, but I am not aware whether or not it is there subject to a similar variation in form. Nilsson observes, however, that the marine species found in those localities are generally smaller than those found in other situations.

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* Journal of the Academy of Natural Sciences of Philadelphia, vol. ii. p. 258.

From this list of exceptions to the general rules which have commonly been regarded as decisive of the localities inhabited by recent shells, and of the nature of the deposits in which the fossil species are found, it is manifest that those rules cannot safely be made use of for practical purposes without considerable reservation.

On some Circumstances connected with the Original Suggestion of the Modern Arctic Expeditions. Communicated by the Rev. W. SCORESBY, B. D., in a letter to the Editor of the Edinburgh New Philosophical Journal.

STATEMENTS having recently appeared in the "Literary Gazette," in an abstract of a paper by Captain Beechey, read before the Geographical Society; and in the Quarterly Review, in an article on Sir John Ross's account of his late voyage of discovery, &c. at variance with, or in contradiction of, the account given by Sir John Ross of my connection with the origination of the Modern Arctic Expeditions,—I consider it but justice to that gentleman, as well as to myself, to put the public in possession of the actual facts respecting the nature and measure of the participation which I really had in the revival of the celebrated question, out of which these curious expeditions appeared to spring.

The statement in the Quarterly Review, to which I refer, occurs in the introductory passage of the first article of No. CVII., wherein (explanatory of the reasons for undertaking the review of Sir John Ross's publication) it is stated :—

> "There are no circumstances, that we are aware of, which should induce us to be silent; indeed, we feel ourselves specially called upon, and for this reason—it was the *Quarterly Review* that took the initiative in reviving and discussing the question of a North-west Passage—of examining the grounds of a probability for its existence—and recommending that expeditions should be sent forth to decide, if possible, a question of itself highly interesting and important, and which had excited an ardent and devoted zeal in the naval worthies of Great Britain, under the fostering protection of Government, many centuries ago."

Then, in a note attached to the commencement of this passage, it is stated:

> "Here we may observe, that, at the very threshold—in his silly 'introduction'—Sir John Ross starts with a misrepresentation. 'It is not generally known,' he says, 'that the question of the North-west Passage, which had been lying dormant since the voyage of Captain Phipps, was, in 1817, revived by Mr William Scoresby,' &c.—that 'he wrote to Sir Joseph Banks, and that on Sir Joseph Banks's recommendation his proposal was attended to,' &c. 'Now this statement,' proceeds the Reviewer, 'is wholly incorrect. Mr Scoresby did write to Sir Joseph Banks, but not about the North-west Passage; he merely acquainted him with the fact of the disappearance of the ice from the coast of Greenland. We happen to know that Sir Joseph Banks never made any recommendation to the Government, nor corresponded with any of the public officers on the subject, except with Mr Barrow, the Secretary of the Admiralty. Mr Scoresby published two volumes, one on the Arctic Regions, the other on the Greenland Whale-Fishery, but not till 1820; and in his remarks on the celebrated question he constantly refers to Nos. xxxv. and xxxvi. of the Quarterly Review."

Now the censure herein thrown upon Sir John Ross, by his generously claiming for me some portion of credit for the revival of the question of the North-west Passage, demands, in common justice, that I should endeavour to substantiate his statement; and, so far as either myself or my friends have yielded him the information from which he writes, that we should be willing to bear our share of the responsibility.

The following sketch of my participation in the revival of the subject of polar research, with the *apparent* influence of my correspondence with Sir Joseph Banks thereon, may serve, I trust, to place the matter fairly before the public.

My personal participation in the revival of the subject in question, was commenced by a letter addressed to Sir Joseph Banks, from whom I had received many marks of kindness, and with whom, for many years, I was in the habit of corresponding.

In that letter, written from Whitby, and bearing the date 2d of October 1817, I mentioned the fact of a large body of the usual ices having disappeared out of the Greenland Sea, and the consequent openness of the navigation towards the west, whereby I was enabled to penetrate within sight of the eastern coast of Greenland, to a meridian which had usually been considered as having become totally inaccessible. After some account of the state and configuration of the ice, and our progress amongst it, I proceeded to remark on the facilities which on this occasion were presented for making researches in those interesting regions. The examination of the coasts of both Spitsbergen and Greenland; the determination of the fate of the ancient colony established by the Icelanders in the latter; explorations affecting the improvement of our whale-fishery; and *researches towards deciding whether or not a navigation into the Pacific by a north-east or north-west passage existed,*—were among the subjects suggested as the most interesting and important. I also expressed a wish to be employed in such researches through a series of voyages, that the most favourable seasons might be improved to the best advantage, and that the most complete investigation might be accomplished; and, by the way of avoiding unnecessary expense, I proposed to combine the object of the whale-fishery with that of discovery on every occasion when the situation of the ice was unfavourable for research.

Whether it was in consequence of this letter or not, it becomes not *me*, perhaps, to hazard an opinion; but there can be no impropriety in stating what actually occurred, namely, that, in the latter end of November, (about seven weeks, I believe, after my letter to Sir Joseph Banks was dispatched), a notice appeared in the public prints of the day, "that, owing to the statements of the *Greenland Captains* respecting the diminution of the polar ice, the Royal Society had applied to Ministers to send out vessels for discovery in the Polar Seas."

In the beginning of December I addressed another letter to Sir Joseph Banks, submitting an outline of objects for research, classed under these several heads:—Investigations for the advancement of geography, commerce, and science; under the latter were suggested as matters of interest, meteorology, including observations on atmospherical electricity; hydrography or natural history of the sea, comprising experiments and observations on its depth, currents, saltness, and temperature both at the surface and at considerable depths; zoology; botany; geology; magnetism, including observations on the variations of the compass, on the magnetic anomaly (or deviation) on ship-board, and on the magnetic intensity.

Just before this letter was forwarded, my father, who was then in London, and had several conversations with Sir Joseph Banks, and other gentlemen, who were anxious for the success of the intended expeditions, was advised by them, and particularly by Sir Joseph Banks, to send for me, with the view of my being employed in this interesting service. Accordingly I left Whitby on the 11th of December, and proceeded direct to London, where I had an interview with Sir Joseph Banks, who, after a kind expression of his regret that he had not been able to obtain for me, as his anxious wish was, a command in one or other of the projected expeditions, referred me to Mr Barrow for the plan on which they were to be appointed. In the course of a brief conversation with the latter gentleman, I was told, that, if I wished to accompany either of the expeditions, I must give in my proposals to the Navy Board. Finding, however, it was a fixed point that the command of all the vessels then designed for discovery, should be given to officers of the Royal Navy, I at once decided, not being disposed to engage in a subordinate capacity, on foregoing the satisfaction I had in some degree anticipated.

Whatever may have been the bearing of any suggestions of mine *on the origination of the recent series of Arctic explorations*, so much at least is certain, that the Quarterly Review is not correct in asserting that Mr Scoresby did not write in his letter to Sir Joseph Banks "about a north-east or north-west passage," but "*merely* acquainted him with the fact of the disappearance of the ice from the coast of Greenland:" And, being mistaken in this important particular, on which Sir John Ross founds his claim on my behalf for some share in the origination of the recent expeditions, from the censure heaped upon him of putting forth "a misrepresentation," and "a statement wholly incorrect," the public will, I doubt not, feel it but just to release him.

That, however, the sketch already given of my letter to Sir Joseph Banks may be duly substantiated, and the nature of that communication more accurately exhibited, I shall subjoin some extracts from a copy fortunately preserved of the letter in question, which, as far as I know and believe, are (*even verbally*) accordant with the original.

After some observations not particularly bearing on the subject in question, the letter states,—"I found on my last voyage about 2000 square leagues of the surface of the Greenland Sea, included between the parallels of 74° and 80° N., perfectly void of ice, which is usually covered with it. Now, all this ice has disappeared within the last two years, and there is little doubt but it has been drifted to the southward into warmer climates, and there dissolved."

After a description of the progress made towards the coast of Greenland, &c. already mentioned, the letter proceeds:—

"Had I been so fortunate as to have had the command of an expedition for discovery instead of fishing, I have little doubt but that the mystery attached to the existence of a north-west passage might have been [I should have added 'in some measure'] resolved. There could have been no great difficulty in exploring the eastern coast of Greenland, and probably the fate of the colony established by the Icelanders so many centuries ago might have been ascertained. *I do conceive there is sufficient interest attached to these remote regions to induce Government to fit out an expedition, were it properly represented.* The simple examination of the shores of Spitzbergen would be a matter of much interest to the naturalist and geologist.

"I should have much satisfaction in attempting an enterprise of this kind, namely, to examine and survey the islands of East Greenland or Spitzbergen, especially the eastern part, which has not been visited [for] many years past; and to ascertain, for the benefit of the whalers, whether the whales resort thither;* to endeavour to reach the shore of West Greenland, determine its position, prove its insularity, and ascertain the fate of the Icelandic colony, together with making researches [contemplating a continuation of the exploration through a series of years] relative to the north-east and north-west passages, &c.; for the performance of which objects, I could point out a method by which the enterprise could be conducted with little, or possibly no, expense to the nation.

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* "Both my father and myself, in the course of last voyage, attempted, though unknown to each other, to explore the eastern part of Spitzbergen but meeting with more ice than is usual in this quarter, our navigation was interrupted."

This would be accomplished by combining the two objects of discovery and fishing.

"Since no one can possibly state [that is, from observation on the condition of the ice in any one season] what opportunity may occur on a subsequent occasion for pursuing a voyage of discovery, it would be well to have this reserve for the reduction of the expenditure, in the event of the opportunity for discovery failing.

"I conceive that an expedition, consisting of two ships, might be fitted out, and all expenses defrayed, for the sum of L. 5000 to L. 6000.* But in case of any whales being taken—and the fishery might occasionally be prosecuted without [particular] detriment to the other object of the voyage—the expenses would be proportionably reduced, and might, possibly, be altogether defrayed thereby."

Such are some of the particulars included in my letter to Sir Joseph Banks,—a letter written many weeks before any public intimation was given of the intention of Government to undertake a renewal of the long-abandoned enterprise of Polar researches. A reply to this letter was received towards the end of October, franked by Mr Barrow, of which the following is a copy.

"DEAR SIR, SOHO SQUARE, *Oct.* 26. 1817.

"I beg you to accept my best thanks for your very intelligent letter, and for your Treatise on the Northern Ice, which has given me a new and far more precise idea of the circumpolar seas than I had before.

"You are aware, no doubt, that an act of Parliament (16th Geo. III., c. 6), offers a reward of L. 20,000 for the discovery of a north-west passage, and L. 10,000 for the ship that shall first reach the 89th degree of north latitude.

"These rewards have not produced a single effort on the part of any whale-fisher to accomplish either of these great purposes; allow me to ask your opinion whether an act offering a thousand pounds for the reaching every degree of latitude from eighty-two to the pole, would be likely to induce the masters of ships to make a trial to reach at least some of the unknown degrees of latitude?

* * * * * *

"I am, Sir, your obliged and faithful servant, JOS. BANKS."

About three weeks after the receipt of this letter, a second, of which I also give a copy, came to hand:

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* This off-hand estimate was not meant to include the owners' remuneration for the use of their ships, but the mere outlay for equipment, provisions, and wages.

"Dear Sir, Soho Square, *Nov.* 17. 1817.

"I hope you have received my letter of October 26, in which I thanked you for the present which you were so good as to make to me of your Essay on the Ice of the Seas about Spitzbergen and Iceland. The more I have considered the facts stated in it, the more I am convinced that the information given in it to the public for the first time, is likely to lead to results highly advantageous to maritime science.

"Major Rennell, who has written so much and so ably on the currents in the ocean, is much pleased with your Essay. If you could spare a copy as a present to him, he will, I am sure, be very thankful; in that case be so good as to direct it to me.

"Allow me to inquire of you what the quantity and nature of the *drift-wood* found on the coast of Spitzbergen is? I think all who have visited that country agree that it is found on the shores in sufficient abundance to supply fuel for melting their blubber into oil.

"On the comparative quantity of drift-wood on the west coast of Greenland, and on that of Spitzbergen, some conjecture may be grounded respecting the probability of the current which sets to the southward in Davis' Strait, and on the east side of Greenland, taking its origin in the east or the west. I do not recollect any drift-wood coming on shore on the coast of Labrador. The abundance is found on West Greenland, which argues a current from the west.

"It appears from your Essay that islands of ice are uncommon in the seas of Spitzbergen; they, however, I conclude, sometimes occur.

"I hope you proceed directly with your intended work on the Polar Seas: I am impatient to see it, after having so much profited by your Essay.

"I beg my best compliments to your father, and am, Sir, your obliged and faithful servant, Jos. Banks."

Besides these two letters, I had much additional correspondence with Sir Joseph Banks, in which the polar expeditions formed an occasional topic; but as my chief communication with Sir Joseph expressly on the subject of these expeditions, was by personal conversations, the documentary evidence on the question of the revival of Arctic research, is principally found in the letters above given.

For bringing forward a correspondence of this kind, relating so much to myself, I trust the circumstances referred to in the outset of this communication will justify me; and that these particulars have not been communicated from any undue desire of assuming credit to myself, will, I trust, appear from the fact, that during the long period which has elapsed since the commencement of the modern polar expeditions, I have never before put forward publicly any of the present statements. Had I indeed seen fit to have urged any claim to public consideration, on the grounds kindly mentioned by Sir John Ross, or any desire of public acknowledgment so generously suggested in the Edinburgh Review, I might, perhaps, have founded a plea on the above and other grounds; and had I been disposed to complain of a want of consideration on the part of those who availed themselves, without acknowledgment, of some of my suggestions; or of those in public offices under Government, who put me to considerable expenses by two journeys to London on the public service, perhaps a case could have been made out, such as to have excited a feeling with the public, in some degree corresponding to that indicated in the two friendly publications referred to, that I had not been dealt with *generously.*

On the Causes of Obstruction in Water-pipes and Syphons from Disengaged Air; and on the Construction of a Hydraulic Air-extractor for Removing them. (With a Model and Drawing.) By J. STEWART HEPBURN, Esq. of Colquhalzie, Mem. Soc. Arts.*

COLQUHALZIE (by CRIEFF),
8th December 1833.

SIR,

I BEG leave to submit to the Directors of the Society of Arts the accompanying paper on the obstruction of water-pipes by air, and the means of removing it. Should the subject be one which they choose to entertain, and the manner of treating it appear worthy of their notice, I shall forward to you a model of the proposed apparatus, and a description of the means of giving it a self-acting power.

It may be proper to mention, that the circumstance which gave occasion to it was my observing that the London Society of Arts had, some years ago, given a premium to Mr Cowan for the application to the syphon of a single air-vessel, from which the air was to be expelled by a powerful force-pump. The double air-vessel, which I had previously projected, appearing to me much more effective, as well as more simple and economical, it occurred to me to submit a description of it to the Directors of the Highland Society, who were pleased to honour it with their thanks, and a place in their Transactions. It was not till very lately that I had a model constructed,—the operation of which, while it confirmed my opinion of its practical utility, suggested some improvements on the design. The present paper contains the statement of the result, particularly as applied to water-pipes, and especially to the means of obtaining the command of water lying beyond a ridge of high land. My chief reason for submitting it to the Society of Arts is, the belief that the approbation of so competent judges (should it appear to deserve it), would be the best means of obtaining for it a trial in practice. Although the application of an air-vessel is not, as I at first supposed, altogether new, this particular mode of its application, and the deduction of the causes of obstruction of pipes by air, I believe to be so, at least I am unable to learn any thing to the contrary.

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* Read before the Society of Arts, 5th February 1834.

I shall be obliged by your recommending the paper to the notice of the Directors. And I have the honour to be, &c.

J. STEWART HEPBURN.

JAMES TOD, Esq. W. S.
Secretary to the Society of Arts of Scotland.

It has been long known that all fluids, exposed to the ordinary pressure of the atmosphere, absorb a portion of the air, which, however, appears to be rather mechanically diffused through the fluid than chemically combined with it, being retained in union with it by a force which is in the conjunct ratio of the atmospherical pressure and the corpuscular attraction of its particles. Accordingly, the quantity of air which any fluid is capable of holding in solution, varies with the degree of pressure to which it is subjected, and with the varying state of its corpuscular attraction. Of the effect of increased pressure in augmenting the capacity of fluids to hold gases in solution, a familiar instance occurs in the case of aërated waters, and of liquors which have been bottled up before the termination of the vinous fermentation. A portion of the carbonic gas contained in the liquor continues to disengage itself, until, accumulating under the cork, it causes such an increase of pressure on the surface of the fluid, that the farther evolution of gas is stopped, the liquor becomes tranquil, and the rest of the gas remains suspended in it. But when, by the drawing of the cork, the accumulated pressure is suddenly reduced to that of the atmosphere, the suspended gas is instantly disengaged with violent effervescence.

The fact of the natural tendency of fluids, under the ordinary atmospherical pressure, to absorb a certain portion of the air, and to allow it to escape on the removal of that pressure, is exhibited with great distinctness and precision in the well-known experiment of the Toricellian tube. If a glass tube, three feet in length, and with one of its ends hermetically closed, be filled with unprepared quicksilver, and then inverted in a perpendicular position with its open end plunged in a basin of the same fluid, the mercury in the tube will suddenly sink to the height of thirty inches (the mean height of a column of mercury equivalent to an atmospherical column of equal diameter), leaving in the upper end of the tube a perfect vacuum. This is proved by bringing the tube to an inclined position; for then the mercury will suddenly spring upwards, until it strike sharply against its upper extremity, falling to its former height when the tube resumes its vertical position. But the surface of the mercury in the tube being no longer pressed by the weight of the atmosphere, the air contained in the fluid will begin to evolve itself; and, rising to the surface, will diffuse itself in the vacant portion of the tube. If, after a few days' repose, the tube be again examined, it will be found that, on being inclined from its vertical position, the mercury, although it will indeed still rise in the tube, will no longer reach the summit; but, on approaching, it will rebound with repeated oscillations, shewing at once the *presence* and the *elasticity* of the air that has been disengaged.

The same fact, of the absorption and disengagement of air, may be shewn in the case of water, although in a much less convenient form, inasmuch as the column of water equivalent to the atmospherical column is no less than 32 feet in height. If a tube of iron or lead, about 34 feet in length, closed at one end, and having (for the convenience of observation) a long slip of glass cemented into its side at the height of 32 feet from the open end, be filled with water and inverted in a reservoir, the water will be seen to sink to the height of 32 feet, leaving a vacuum above, which may be proved in the same way as in the mercurial column. Speedily, however, minute bubbles of air will appear in the water; and, rising to the surface, will, as in the former case, occupy, in a high state of rarefaction, the vacant portion of the tube.

The same phenomenon is exhibited in the action of the syphon; and the description of it in this form brings us nearer to our purpose. Suppose, then, a leaden pipe of 70 feet long bent into the form of a syphon, having the shorter limb, of 34 feet in length, plunged into a deep tank or reservoir to the depth of 30 feet, and the end of the longer limb immersed in a small vessel of water (see fig. 2.) attached to, and moveable along with it, in order to prevent the admission of air during the varying action of the syphon. If the air be now exhausted from the syphon by a syringe applied to the end of the longer limb, the atmospherical pressure on the surface of the water in the reservoir will force it to rise in the syphon, and, descending the longer limb, it will issue in a full stream. If the syphon be then gradually elevated, the water will continue to flow from it, but with less and less velocity; because, while the atmospherical pressure which forces it to rise in the syphon is constant and uniform, the gravity of the ascending column goes on augmenting with its increasing length, until, at the height of 32 feet, the two forces come into equilibrium, and the water consequently ceases to flow. The syphon will, however, still continue full, because, both ends being immersed in water, no air can enter to displace the fluid contained in it; and if the syphon is again lowered, the water, obedient to the renewed atmospherical pressure, will again begin to flow. But if, in place of being *lowered*, the syphon is *raised* to the height of 33 or 34 feet, the two columns will separate at the summit, and each will fall in its own limb to the height of 32 feet above the surface of the water in which its lower end is immersed, leaving a perfect vacuum in the upper part of the syphon. If the syphon be immediately depressed to its former position, the columns will again unite and begin to flow; but if retained in its elevated station, the vacancy in the upper part will, as in the case of the vertical tube, be in a short time occupied by the rarefied air which evolves itself from the water in consequence of the removal of the atmospherical pressure; and if, after this has taken place, the syphon be once more lowered to its original depth in the reservoir, the water will indeed again rise until the rarefied air above it is compressed into the same density with that of the atmosphere; but the air will maintain its place in the upper part, and the flow of the water will not be resumed. This evolution of air in the syphon occurs not merely when the diminution of atmospherical pressure approaches its extreme limit, but more or less in all circumstances. For, at the summit or vertex of the syphon, there is not only a certain diminution of pressure, even when its elevation is small, but the gravity of the descending column in the longer limb tends to weaken the cohesion of the fluid, and to bring into play another co-operating cause of gaseous development, which shall presently be explained. So that, in this instrument, the causes of accumulating obstruction are constantly at work, lessening its utility, and greatly limiting the application of which it is really susceptible to various useful purposes.

It has been stated in the outset, that the capacity of fluids for retaining air in solution depends not only on the amount of atmospherical pressure, but on the varying degree of the corpuscular attraction of its particles; and every cause which weakens that attraction tends to the disengagement of the combined air. The expansion of fluids by heat is an agent of this description, and as it is one of convenient application, it is accordingly employed for this purpose, both in chemistry and the arts, and particularly for expelling the air from quicksilver in the construction of the barometrical tube. In the case just detailed, both of these causes appear to be in operation. For, while the sustaining atmospherical pressure operates with less and less force on each successive film of the ascending column of water, the counteracting force of gravitation is, practically, the same on all, operating with the same intensity on the highest as on the lowest film;—and it acts with still greater and more undivided force on the descending column in the longer limb, in a manner yet more direct and palpable, causing the fluid to descend with a f[illegible]'ly accelerating, while it is ascending in the lower limb with a force more and more retarded. This double action, therefore, of the gravitating force in the syphon tends to weaken, and at last to overcome, the corpuscular attraction in the middle part or summit of the double column, as we have seen actually to take place as soon as the weight of the column of water came into equilibrium with that of the atmospherical column.

A proof of the important share which the diminution of the corpuscular attraction has in causing the evolution of the combined air, is to be found in what takes place in the case of water flowing through conduit pipes along a declivity of considerable but unequal inclination. In this case the water, although it enters the pipe with only the force corresponding to its depth beneath the surface, that is, to the pressure of a hydrostatic column of the diameter of the pipe, and of a height equal to its depth (*a* B, fig. 3) below the surface of the water, yet tends [*] to issue from the lower end with the velocity due to the total height of its fall (or in the proportion of the square root of the height *c* D, to the square root of the height *a* B), modified by the friction of the pipe, which, however, according to the experiments of Bossut, is neutralized when the inclination of the pipe amounts to 6° 31′, or about 1 in 8. It would appear, therefore, that the intermediate part of the moving column must be, as it were, in a high state of *tension*. As its velocity increases it exerts less and less pressure on the sides of the pipe, and receives less pressure in return,—the increasing momentum of the descending column, particularly of the interior filaments, which are not affected by the friction of the pipe, tends to elongate the mass, to diminish its diameter (even to the verge of creating a vacuum between it and the sides of the pipe), and to weaken its corpuscular attraction, which is now sustaining almost the whole action of the gravitating force; just as, when it issues from the mouth of the pipe, its velocity progressively increases, its diameter contracts, and its corpuscular attraction diminishes as it falls towards the earth, until, if the height of its fall be great, its cohesion is at length entirely lost, and it is divided and scattered into spray. That this diminution of the corpuscular attraction really has the effect of decreasing the capacity of the fluid for retaining the combined air, appears from the fact, that, in the circumstances described, the air is found to be gradually disengaged, and to lodge in those parts of the pipe which have the smallest degree of inclination; in which situation its natural buoyancy has less power to force its way against the opposing action of the current, obstructing by its presence, or entirely stopping, the flow of the water through the pipe.

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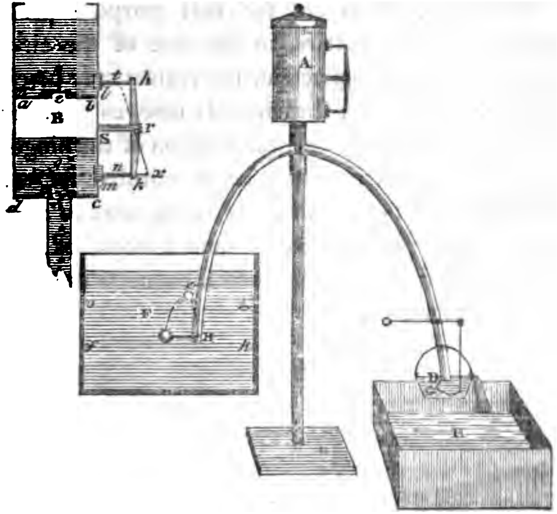
[*] I say *tends* to issue, because the affirmation of some writers that in long horizontal pipes the water *enters* the pipe with much *greater* velocity than it leaves it, must be taken with some qualification. For, if this were strictly true, it is evident that an accumulation and condensation must take place in the interior of the pipe, which would quickly attain a maximum, and prevent the water from entering materially quicker than it was discharged. And, on the other hand, its tendency in pipes of great inclination to *issue* with greater velocity than it enters is controlled by the increased friction of the pipe, and by the corpuscular attraction of the water, which, together with the pressure of the atmosphere at the lower extremity of the pipe, prevent the continuity of the column from being broken, as it would be, did the water actually issue with much greater velocity than it enters, and reduce its final to very nearly its initial velocity.

This tendency of the disengaged air to lodge in the horizontal parts of water pipes, and in the top of draining syphons, often occasions much trouble and inconvenience; and the ordinary means of expelling it even from water-pipes by air-cocks and the forcing syringe, appeared to the writer of this paper so inconvenient and ineffective, that he conceived the idea of the application to his own water-pipes of a double air-vessel for extracting the imprisoned air. This attention being afterwards drawn to the draining-syphon, in which the difficulty of expelling the confined air is still greater, and the means of effecting it still more imperfect, this air-vessel appeared to him no less applicable to the syphon than to water-pipes; and, after some experiments with a working model, it assumed the form which he is about to describe, and which he is persuaded will prove effectual in practice for both purposes.

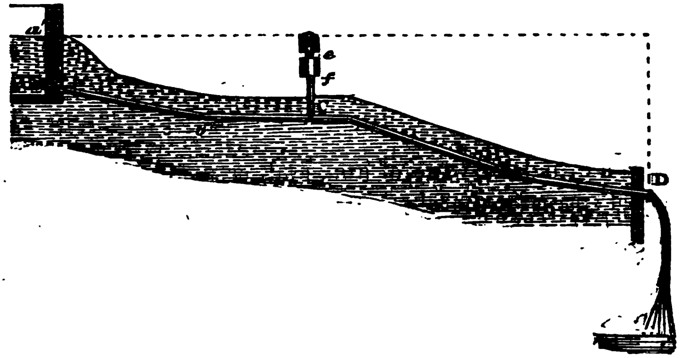
Fig. 1. Fig. 2.



AB is a cylindrical vessel of iron, lead, or copper, divided into two equal spaces by the false bottom *a*, *b*, containing the air-tight valve *c*; the lower bottom *d*, *e*, contains a similar valve *f*, and, in order that this valve may be accessible for being cleared or repaired when necessary, the bottom plate containing it may be made to screw into the cylinder. To the under side of the valve *f* is fixed the vertical pipe C, of such length as may be necessary to connect it with the syphon or water-pipe to which it is to be applied (see fig. 2 and 3). An upright pin *p*, *q*, fixed in the top of these valves, passes through oblong holes in the sliding brass rods *gh*, *ik*, which work horizontally, passing through water-tight leather collars in the stuffing-boxes *l*, *m*, in the sides of the cylinder, and through holes in the knee-plates *n*, *o*, fixed to the interior opposite side, by the alternate motion of these rods the valves are opened and shut. A simultaneous alternate motion is given to these rods by the lever *h*, *r*, *k*, turning on its center *r* on a pillar *r*, *s*, fixed on the side of the cylinder; and, by means of vertical hinge-joints in the rods at *t*, *u*, their free parallel motion is secured. In the position of the lever represented in the figure, the valve *e* is shut and the valve *f* open; by a small turn of the lever into the direction *l*, *x*, the valve *f* is shut, and the valve *e* opened.

The movements of the valves may also be managed by cranks fixed on their axes, which are for that purpose prolonged, passing through leathern collars, in the side of the air-vessel; and to obtain a simultaneous action, the cranks may be connected by a working rod; only, that in this case, as the axes must be in the same perpendicular line, the position of the hinges must be reversed. Stop-cocks may also be substituted for valves in the case of water-pipes;—in which case, there are two air-vessels connected by a short pipe, which contains a stop-cock, and this stop-cock being fixed in the pipe which connects the lower air-vessel with the water-pipe.

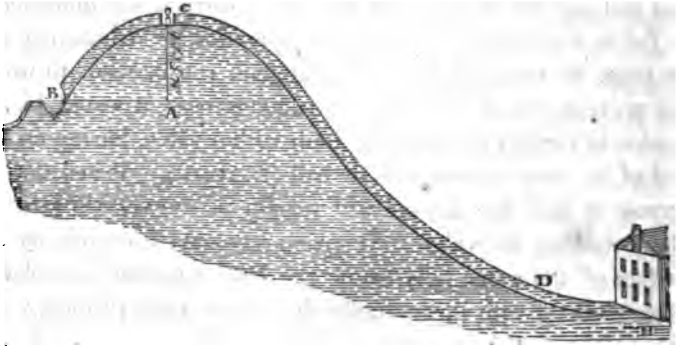
Fig. 3.



The air-extractor, thus constructed, is applied to use in the following manner. The pipe C, fig. 1., the upper end of which is connected with the lower valve *f*, has its lower end inserted in that part of the syphon or water-pipe in which the air is found to lodge; and the cylinder being filled with water, the upper valve *e*, is shut, and the lower valve *f*, is opened. Under this arrangement, it is plain, that whenever any air accumulates at the bottom of the pipe C, it will immediately rise through it into the lower cylinder B, displacing an equal bulk of the water, which will descend through the pipe C, and be carried off by the moving water, which, relieved of the air, is flowing without obstruction. When the air has again accumulated, so as to fill the cylinder B, and pipe C, the obstruction is found to recur; and in order to get rid of it, the communication between the cylinder, and the pipe or syphon, must first be cut off, by shutting the lower valve *f*. The upper valve is then opened, when the water in the cylinder A will descend, and displace the air in the cylinder C, which will rise through the upper valve, and pass off into the atmosphere. It will now be found that the obstruction is removed, and that the water flows freely. Then both cylinders are to be again filled with water; after which the upper valve is shut, and the lower one opened as before. By the connecting lever these movements are made simultaneously.

In practice it may possibly be found necessary, at least when the air has been allowed to accumulate in the pipe, while the valve *f* is shut, to interrupt, for a moment, the entrance of the water into the mouth of the pipe or syphon, in order to allow the air to rise freely into the cylinder. The reason of this seems to be, that as soon as the opening of the lower valve, and the ascent of the first bubble of air partially relieves the water in the pipe from the pressure of the confined air,—the renewed impetus of the water, in attempting to resume its motion, forces the air forward, and partially prevents its ascent through the valve into the cylinder. It may be also observed, that, in the draining syphon, it will be advantageous to prevent the access of the atmospherical air at both its extremities: for if the water in the reservoir or quarryhole is allowed to drain off until its surface descends to the extremity of the shorter or drawing limb, the air will instantly rush up and empty the syphon. And, again, if, while its action is suspended by the accumulation of disengaged air within, the lower or discharging extremity of the longer limb be exposed to the atmospherical air, it will ascend, and dislodge the water from that limb, when it will immediately fall in the other also; and the operation of exhausting the air must be repeated before the syphon can be made to work. To prevent the first of these inconveniences, it would be advisable to furnish the drawing limb with a valve, having on the end of its axis a lever and air ball (B, fig. 2). When the reservoir is full, the air-ball will remain in the vertical position B *e*, retaining the valve fully open; as soon, however, as the surface of the water falls below the level *e g*, the air-ball will descend along with it, gradually closing the valve; which, when the surface of the water, and, along with it, the air-ball, has reached the level *f h*, is entirely shut, preventing both air and water from entering the syphon until the water again accumulates, raising the air-ball along with it, and opening the valve. The second of the inconveniences referred to, is to be remedied by keeping the end D immersed in a trough or vessel G; which, receiving the water as it flows from the syphon, remains always full, and prevents the entrance of air. Thus furnished with an air-extractor, and the means just described of preventing the water from being drained off, all that is necessary to renew the working of a syphon obstructed by the presence of disengaged air in its vertex, is to close the valve B, by pressing down the air-ball, if in practice that be found necessary, and then to press down the end *h* of the lever *h k*, (fig. 1.), which, shutting the valve *f*, and opening the valve *e*, allows the water in the cylinder A to descend, and the air in the cylinder B to escape. The air-ball being then allowed to rise, and the valve B opened (fig. 2.), the water will begin to flow; when the cylinder A may be again filled with water, to be in readiness when required. In situations where, in setting the syphon to work, it may be difficult to employ a syringe for exhausting the air, the purpose may be served by furnishing the lower limb D also with a valve; then, shutting both valves B and D of the extremities, and opening both valves of the air-extractor (by disengaging one end of the lever from the working-rod), fill the syphon with water from the top; when this is done, all that is necessary is, to shut the upper valve of the air-extractor, and open the valves B and D, when the water will flow freely.

Fig. 4.



When this mechanism is to be applied to free conduit-pipes of air it is to be inserted into the place where the air is most disposed to lodge, being that part of the pipe which has the smallest degree of inclination (Fig. 3.); then, as in the case of the syphon, to shut the stopcocks at B and at *f*, and open the stopcock *e*, when the air which has accumulated from the pipe into the lower cylinder will escape, and the water in the upper vessel descend to occupy its place. Then the valve *e* is to be shut, and *f* and B to be opened, and the upper cylinder again filled with water until the operation requires to be repeated. In water-pipes these operations must be performed by the hand of the person in charge of them; but, in the case of the syphon, a self-acting power may easily be given to it, which will work the valves at intervals without the necessity of attendance.

It now only remains to describe a certain case or class of situations in which the air-extractor may be applied to a purpose of some importance. Suppose the house H, (Fig. 4.), is so situate as to be unable to obtain a spontaneous supply of water, but that a constant spring B is discovered on the opposite side of a hill or long ridge of high land A, the lowest point C of whose summit is thirty feet higher than the spring. No doubt if a pipe were laid from the well over this lowest summit to the house, the water would, on the air being extracted from the pipe, flow to the house, the pipe acting as a syphon. But the accumulation of disengaged air would in a very short time create such an obstruction as could not be removed by any of the means at present in use; and, if ever attempted, it would probably be abandoned as hopeless. This, however, is a case to which I apprehend the air-extractor might be applied with a certainty of success; and it is in situations of this kind that I think it especially calculated to be of practical utility: for I am persuaded that many fine springs of water, which at present would be considered as utterly inaccessible, might, by this means, with ease and certainty, be commanded and applied to useful purposes. Nay, by employing several pipes or syphons of large size, and uniting their vertices by connecting-pipes with one common air-extractor, a supply of water, in larger quantities, might be obtained from springs or rivulets in similar situations, for irrigation, machinery, bleaching, and other purposes, when unattainable by ordinary means.

Colquhalzie, *2d December* 1833.

Report by Mr DUNN.

Mr Stewart Hepburn is correct in stating that fluids hold a quantity of air in solution, or mechanically combined with them, although I am afraid his proof, deduced from the filling of a barometer-tube, will not hold good, as indeed the expression he makes use of, " unprepared mercury," indicates itself. Mr Hepburn is perhaps not aware, that a very large proportion of all the barometers made are filled simply by pouring the mercury into the tube. It is only the more carefully made ones that have the mercury boiled in the tube. I would find no difficulty in filling a tube either to produce the effect or not, as I had a mind, with the same mercury.

Mr Hepburn's paper contains many valuable hints as to the causes of the obstruction to the flow of water in pipes which, although not new, are highly valuable and by far too little known. There is a very remarkable similarity betwixt Mr Hepburn's invention and one submitted to the Edinburgh Water Company, or Magistrates of Edinburgh, by Mr Hunter of Thurston, in 1820, where Mr Hunter suggests a double air-box, precisely the same as Mr Hepburn's, to be applied to a syphon to rise over the height of George Street, instead of tunnelling. It is, however, I think certain, that the idea was original to both, and affords a strong presumptive proof, not only of its being desirable and useful, but also that it is the best way of effecting the object in view. Upon the whole, I think Mr Stewart Hepburn deserves the best thanks of the Society for his ingenious, and I will say, notwithstanding the remarks I have made above, original paper. I should have said that Mr Hunter proposed to open his boxes by stop-cocks.

Report by Mr GEORGE BUCHANAN.

COCKBURNSPATH NEAR DUNBAR,
22d April 1834.

In regard to Mr Hepburn's paper, I have not had time to get a regular report completed, but sent you the paper this morning, in which there is a draft of report, and also notes by Mr Dunn. In addition, I would make the following remarks:— Mr Hepburn's paper I consider an ingenious and well written essay on a very important subject in practical hydraulics, and the thanks of the society are due to him for it, and particularly for his contrivance for drawing air off from the syphon. In regard to his views of the absorption of air by water, and its disengagement in pipes, I cannot just altogether agree or follow him in his notions; but the subject, it must be confessed, is not sufficiently understood. The idea of a mechanical mixture of the air with the water I conceive quite inadmissible; and, when we consider that the former is 800 times lighter than the latter, it is obvious they must be united by a very powerful attraction overcoming the natural buoyancy of the air. When the pressure of the external air is removed, the particles confined within the water are seen to expand. Their buoyancy, therefore, increases, and they overcome the force of attraction with which the buoyant force was forming in equilibrio; and this is the true cause of their disengagement in the upper parts of the syphon. The diminution of corpuscular attraction, on which Mr Hepburn lays so much stress, I cannot think has much influence, as this, in any case, is so extremely weak a force. In regard to the disengagement of air in pipes, the curious circumstance here is, that this takes place without the pressure of the atmosphere being removed at all, and even when it is greatly increased by all the pressure of the column of water to the height of the fountain head. This is a very curious fact indeed, and not easily explained; but the thanks of the Society are certainly due to Mr Hepburn for bringing this subject under their notice, as it may lead to useful discussion.

Report by Mr Clerk Maxwell.

Many eminent engineers have been disappointed in their expectations of the quantity of water conveyed by pipes, owing to various causes of obstruction to the regular flow, and which make the result different from that deduced by calculation on the most approved theory. The essay of Mr Hepburn is therefore of much value in elucidating the obstruction caused by air in pipes, and in suggesting a method of removing it; and in particular, in pointing out a method of withdrawing air from the syphon, and so rendering it a much more valuable instrument than it has hitherto been. The same method of doing so (although undoubtedly original to Mr Hepburn), was pointed out (but not acted on) by Mr Hunter of Thurston, in a letter to the Directors of the Edinburgh Water Company, in January 1820. The circumstance of two persons having separately arrived at the same conclusion, marks strongly the advantages of the plan proposed.

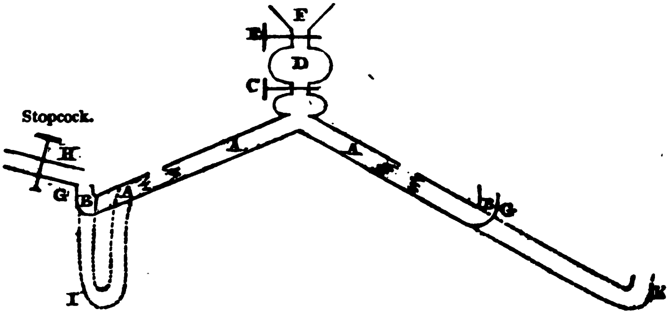
It may perhaps be unnecessary for the Committee to examine minutely all the propositions in the essay, as to the air being disengaged from being relieved from the pressure of the atmosphere, and from the diminution of corpuscular attraction (which is perhaps only a different term for the effect produced by the former cause), but they consider the main fact of the extrication of air in pipes satisfactorily elucidated.

The self-acting apparatus would no doubt answer the purpose intended, but probably in most cases the necessity for it would not occur, as the letting off the air can be so easily done by the hand, and being periodically required, could be done at little or even no expense; or, if an extraneous supply of water at the highest point of the syphon could be obtained, some more simple machinery might be made to effect the movement of the valves.

On the whole, the Committee are of opinion that the approbation and thanks of the Society should be tendered to Mr Hepburn for his ingenious contrivance of the double air-vessel, and for his valuable essay.

Copy Letter from JAMES HUNTER *of Thurston, to the* CHAIRMAN *of the* WATER COMPANY, *Edinburgh, dated 20th January* 1821, *referred to in the previous Report. With a Drawing.*

SIR,—I observe operations going on at the north end of the Mound, which I understand are the commencement of a tunnel upon a dead level below George Street to Queen Street, for conveying the new supply of water to the lower parts of the New Town. As this work (if I am correctly informed on the subject) will cost a great sum of money, I think it ought to be considered whether the following plan will not be a very great saving. I should think George Street is not thirty feet higher than Prince's Street, therefore, a syphon laid near the surface of the ground will act; or, at all events, in case of necessity, it will be easy to sink it a few feet at the top, so as to bring it within the range of thirty feet, but as a syphon is apt to gather air at the top, I propose a contrivance, which will effectually remedy that inconvenience.



AA are two legs of a syphon.
BB, the two ends turned up, so that the water cannot run out at either end.
C and E, stopcocks.
D, a ball containing about a cubit foot.
F, a funnel.
GG are upon the same level.
H is the pipe bringing water to Prince's Street.

To carry this plan into effect, shut up the two ends BB, and open the cock C and E. Pour in water at F till the syphon is quite full, then shut both cocks, leaving D and F full of water, and open the ends BB.

The syphon BAAB will not only constantly run when supplied with water, but will continue charged though the supply should be stopped. However, as a good deal of air is constantly separating itself from water in pipes, it will of course collect immediately below C; and to get rid of this, it will be necessary to let it out, perhaps once a-week, but for safety it can be let out every second day. I propose the following method. Open the cock E, and fill D with water; next shut E and open C; the water in D will immediately descend into the syphon, and the collected air rise into D; again, shut C, and if necessary, repeat the operation, always finishing by leaving D and F full of water. As the opening of both cocks at once would empty the syphon, it will be necessary to fasten the same handle to both cocks, in such a way, that opening one shall shut the other, but that the second one may also be shut separately.

This syphon will run fully as fast as a straight pipe, but if there is any level to spare between Prince's Street and the end of the pipe near Queen Street, the advantage of it will be obtained by sinking B in Prince's Street straight down, and turning it up again towards H, so that the bend I of the pipe shall be as low as the bend K in Queen Street.

It is to be remembered that many of the wells of the New Town may be dried up by coming upon a spring in boring through the hill, which is not at all unlikely. The practicability of the scheme may be put to the test for less than L.3.—I have the honour to be, &c.

THURSTON, DUNBAR, *20th January* 1821.

On a Curious Phenomenon observed in the Island of Cephalonia, and on the proximate cause of Earthquakes in the Ionian Islands. By JOHN DAVY, M.D., F.R.S., Surgeon to the Forces. In a Letter to Professor FORBES.

FORT PITT, CHATHAM, *July* 2. 1835.

MY DEAR SIR,—As I fear it will not be in my power to attend the meeting of the Association for the Advancement of Science next month in Dublin *, allow me through you, to call the attention of the physical class to a phenomena in Cephalonia, of which I have lately been informed, of an extraordinary and novel kind, as it appears to me, viz. the occurrence of streams or currents of salt water *from* the sea into the land.

This curious fact was first mentioned to me, some months ago, by my friend Dr White, Assistant Surgeon of the 2d Battalion of the Rifle Brigade, in a letter which I received from him when I was in Malta. I lost no time in replying to him, and begged him to collect as much information as possible on the subject, urging him at the same time to publish an account of the phenomena. He has in part complied with my wishes; this morning I have been favoured with a second letter from him, giving the results of his inquiries, and requesting me to make any use of them I may think proper. I cannot do better, therefore, in relation to the object which I have in view, than to give his description in his own words, extracted from the letter just referred to, which is dated Ithaca, May 26.

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* It will be seen by the letter that this paper was intended to have been communicated to the British Association; but in consequence of Professor Forbes's absence in Spain, was not received in time, and has subsequently been read at the author's desire before the Royal Society of Edinburgh.

" About a mile and a half from the town of Angostoli, in Cephalonia, near the entrance of the harbour, there are four descending streams of sea-water. Three of them are near to each other; the fourth is distant about four hundred yards. Their existence has been long known to the inhabitants of Cephalonia, but the particular period of their discovery is uncertain. No one possessing this information, it appears, had intelligence enough to be struck by the phenomena, or sufficient ingenuity to avail himself of them for useful purposes, until about twelve months ago, when the attention of Mr Stephens, lately collector of customs in Cephalonia, was directed to them.

" This gentleman, soon appreciating the probable value of such streams in the vicinity of the two principal towns in the island (Angostoli and Lixuri), lost no time in obtaining from the local government a lease of them for a certain number of years, and is at this moment erecting a grist-mill on the site of one of them, with the almost certain prospect of success. Hence he has dug a trench of sufficient depth and breadth to receive the frame-work of his building, and to obtain the necessary height of perpendicular fall. This, which varies somewhat with the occasional rise and fall of the sea (chiefly caused by the peculiar formation of the harbour and prevailing winds), Mr Stephens confidently calculates will seldom be under two and a half feet, and that the apertures which communicate with the trench will freely deliver two hundred square inches of water, flowing in at the rate of fifteen feet per second.

" It is a curious fact that streams of fresh water are constantly flowing through numerous fissures into the trench, and in a direction apparently opposite to that which the sea-water takes in escaping; for, whenever this (the water from the sea) is prevented from rushing in, the water in the trench invariably rises a considerable number of inches higher than it does at any other time, and is brackish to the taste.

"The streams are constant throughout the year, except when the mouths of their channels get accidentally choked up with sea-weed, which collects in large quantities in the harbour of Angostoli.

"On commencing their descent no sound is observable, but through the apertures which open into the excavation made by Mr Stephens, numerous bubbles of fœtid air are at times disengaged. This fact has but lately been observed, and takes place only when the quantity of water escaping is small.

"Mr Stephens has been present at his trench when sharp strokes of earthquakes have occurred, but did not observe any particular effect or changes result therefrom."

Thus far Dr White. I may add from my recollection of the locality, that where the streams descend, the shore is low, and composed chiefly of calcareous freestone of very recent formation, hollowed into caverns here and there by the action of the waves.

Reflecting on the nature of the currents described by Dr White, it is hardly possible to refrain from speculation; and, indeed, it is in connection with their theoretical bearing that they are chiefly interesting.

In two points of view they appear particularly important, and may be deserving of especial consideration. The first is in connection with the theory of the formation of beds of mineral salt; the second with that of the production of earthquakes.

I would wish at the present time to call the attention of the Class chiefly to the latter, hoping that some of the able inquirers who compose it, may follow up the subject, and investigate it experimentally.

My idea is, that the earthquakes to which Cephalonia, in common with most of the Ionian Islands, is so very subject, may be owing to the descent of water into the earth, in situations where there are great beds of clay or marl beneath the earth's surface, and between porous and fissured strata, such as of freestone, allowing of the ready access of this fluid. I am disposed to believe that clay or marl in absorbing water expands, and consequently in drying contracts in all its dimensions. The few experiments I have made with a coarse apparatus, the best I could procure in Malta, were in favour of the conclusion. I may menti [illegible] Small cylindrical masses of calcareous marl, which falls to powder in water, of that kind which prevails in the Ionian islands, in Sicily and in Calabria, after having been dried by exposure to the air, was put into a vessel, having two apertures with stopcocks, by means of one of which small quantities of water might from time to time be admitted, whilst the other communicated with a bent tube containing coloured spirit of wine, which served as a gauge to measure any expansion of the air or other materials in the vessel. This prepared apparatus was left several hours undisturbed in a room without a fire, to allow of every part of it acquiring the temperature of the room; the first stopcock was suddenly turned, and water admitted, and, after a few seconds, again closed. Now, watching the gradual absorption of the water by the masses of marl, and their falling to powder, I witnessed a decided, though not very considerable, rise of the spirit in the bent tube, in the direction indicating an expansion within the vessel. Various trials were made with the same general result: the details I cannot give, for my note-books have not yet arrived from Malta, nor indeed is it necessary, for they would be tedious, and, to those acquainted with such inquiries, perhaps of little moment. It may be sufficient to say that I was tolerably satisfied the expansion indicated arose from no other source than the absorption of the water by the clay, and that there was not to my knowledge any cause of error present to vitiate the conclusion; however, as there might have been, I hope the experiment will be repeated, and on a scale in regard to magnitude, commensurate with its importance.

Allowing the result to be correct, that clay or marl actually expands from the absorption of water, the application of the fact is obvious in relation to the production of earthquakes, in situations, as already mentioned, in which there are large beds of these substances under the earth's surface. The volume of these beds undergoing change equally, from the less absorption and loss of water, they may be a constant cause of motion, or of earthquakes, wherever they exist, the motion varying in degree according to a variety of circumstances, which it may be difficult to appreciate, as the quantity of water absorbed, the time in which it is absorbed, the pressure of superincumbent strata, &c. and the accidents connected either with the effects of pent up water (itself perhaps an occasional cause of earthquakes), or with the falling in of great masses of clay, supposing, as there is every reason to believe, that in regions subject to earthquakes there exist extensive subterraneous cavities. You will naturally ask, how do the facts which are best ascertained respecting the localities of earthquakes in the Ionian islands, and their phenomena, accord with this idea?

To me they appear to accord so well as to be a confirmation of it. I shall notice some of the facts which I conceive to be most important and unquestionably authenticated.

1. Earthquakes are most common in the clay districts, as in the low parts of Zante, which consist chiefly of grey marl; in most parts of Santa Maura, where the same marl is abundant; in the low parts of Cephalonia, especially the neighbourhood of Lixuri and Angostoli, also abounding in marl; and in the district of Alleschimo, in Corfu, which consists almost entirely of marl.

2. Earthquakes are rarely felt, and slightly, in those parts of the islands which consist chiefly of rock, whether of mountain limestone, as the mountainous parts of Zante and Cephalonia; or of mountain limestone and clay-slate, as the loftiest part of Corfu, or of the same rock associated, on a small scale, with primitive marble and granite, as in the most elevated region of Cerigo. I would beg to dwell a little on this point of locality. Zante, in the different regions just alluded to, offers a very striking contrast. The frequency of earthquakes in the town of Zante, and its immediate neighbourhood, is universally known. In the summer and autumn of 1824, which I chiefly spent there, hardly a day passed without a shock being felt, and almost every house bore marks of having, more or less, suffered from them; and this town is situated on marl, and backed by lofty hills of the same substance. The exemption of the mountainous regions, composed of limestone, constituting nearly half of the island, is but little known, if at all. When I travelled through it the same year, I did not fail to make inquiries on the subject, wherever I went; and I was invariably answered, as I have just stated, that earthquakes there were rarely indeed felt, and only very slightly. In Santa Maura, it is deserving of notice, there is one village which has always been remarkable for exemption,

and which has escaped uninjured when every other village in the island has been more or less ruined. This is the little village of Frini, situated on strata of limestone, lightly inclined, elevated above the adjoining lowlands, about 600 feet, and presenting towards them several precipices. I visited Santa Maura a few weeks after the destructive earthquake of 1825. I explored the whole island, and Frini was the only village I found which had not then suffered. The shock was felt there, but not a single house was thrown down, whilst in the neighbouring town of Amaxachi, or Santa Maura, as it is more commonly called, hardly a single house was left standing.

3. The Ionian Islands are very peculiar in relation to the distribution and rise of water to the surface. The marl districts subject to earthquakes abound in springs. The limestone districts exempt from them are destitute of springs, and would be uninhabitable without tanks in which rain-water is collected and preserved for use. Near the sea, apparently without distinction in relation to the kind of ground, copious springs, some of them saline, often burst forth, and occasionally even in the sea.

I have now pointed out the principal circumstances which to me seem to favour the idea I have formed of the probable origin of these earthquakes. You will ask, perhaps, Why prefer this notion to the commonly received hypothesis which connects them with volcanic action? I may reply, that the two supposed causes are nowise incompatible; that they may act either separately or together; that probably the most violent earthquakes are owing to the volcanic cause, the slighter and partial, such as are witnessed in the Ionian Islands, to the cause I have imagined. I am farther led to this conclusion, by not having been able to discover, in any part of the Ionian Islands (and I have explored the whole of them with care), any traces of volcanic fires, any traces of trap-rocks, or a single spring, the temperature of which was above the mean annual temperature of the spot where it rose. This is negative evidence, as it appears to me, of a very strong kind, against the earthquakes of these islands having a volcanic origin—especially the fact of the entire absence of warm springs. It seems hardly possible that their cause can be volcanic, without some indication through the medium of springs of the proximity of volcanic fires.

To revert to the phenomenon of the streams of sea-water flowing into the earth,—in connecting them with the production of earthquakes, according to the notion I have started,—of course, I do not consider them more concerned than so many descending streams of fresh water; the water, not the salt, being supposed to be operative. They arrest attention, however, more, and incite to inquiry and speculation more from their singularity, and as apparently denoting vast space below the surface appropriate to the operation of water, in the manner which I have imagined.

When reflecting on the probable influence of marl under the action of water, I was led to make some experiments on marl and clay, and different substances, in relation to their penetrability by this fluid. I shall mention briefly some of the results, as they appear to me to possess some interest, especially in connexion with the matter of inquiry under consideration. The form of the experiment was very simple. A glass tube was selected about an inch in diameter, and about two feet long, which was covered at one end with linen, and placed perpendicularly. It was filled with the substance to be tried, about two-thirds, and water was poured into the empty portion. In the instances in which freestone, or marble in powder, or even carbonate of lime, procured by precipitation, were used, the water penetrated rapidly; some reached the bottom and flowed out in drops; in a few hours it was drained off, the substance tried merely remaining wet. In the instances, on the contrary, in which marl or clay was used, introduced in powder, the water penetrated with extreme slowness. I have not by me the notes of the experiments to consult; but I very well recollect that, after three or four weeks, it had not penetrated into the marl more than an inch. When these substances, in a coarser form, were employed, as in small masses, then of course the descent of the water was more rapid; but no sooner had the marl become swollen from the absorption of water, and reduced to powder, than it arrested the farther rapid descent of the fluid, and the experiments immediately became similar to that first mentioned.

It was my wish to have continued and multiplied these experiments; but circumstances which it would be tedious to mention, unavoidably prevented me, and I am doubtful now if I shall resume them. The results are of easy application; whilst they shew how clay and marl may act in arresting the descent of water, and in producing springs, they also illustrate how, if water has access to great masses of clay or marl, full of fissures, so that it may act on a very extensive surface, a large quantity of it may be absorbed, and therefore it may be supposed a considerable effect of expansion may be produced.

In conclusion, I will only add, that if any of the remarks I have made, or the conjectures which I have ventured to throw out, should happily lead to the discussion of so interesting as well as mysterious a subject as earthquakes, I shall not consider them offered in vain, more especially should they induce, as I hope they may, any one to investigate the subject experimentally. I remain, &c.

J. DAVY.

Account of the Great Suspension Bridge at Fribourg.

THE town of Fribourg is built on the left bank of the *Sarine.* Both sides of this small stream are very steep, and rise to the height of about 220* feet above its bed; and travellers coming from Berne to Fribourg were formerly obliged to descend the hill, in order to reach a small wooden bridge which crosses the river, and immediately after by a steep ascent of about 200 feet to reach the top of the opposite bank before coming to the centre of the town. The passage through Fribourg thus occupied nearly an hour; but the case is changed since the erection of the new suspension bridge.

These difficulties and delays were long considered the unavoidable consequence of the local situation of the town, until some bold spirits conceived the idea of uniting, by means of a suspension bridge, the steep banks of the Sarine. It was necessary that the bridge should pass over a great part of the town itself, and the scheme was considered completely Utopian; yet certain of the authorities and some active citizens determined to submit the measure to the consideration of engineers of different districts. Various designs were accordingly offered, and the Government of the Canton gave the preference to that of M. Challey of Lyons, whose plan has since been executed under his immediate superintendence.]

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* All the measurements have been reduced from French to English, agreeable to the ratios given in the Annuaire du Bureau des Longitudes for 1

The gateways at either end of the bridge are of Doric architecture, and are about 65 feet in height. The tops of their arches are about 42 feet above the roadway, and the arches have a span of 20 feet. The masonry of the gate is 46 feet in width, and its thickness is about 20 feet; and, although the largest blocks of the hard limestone of Jura were employed in this work, iron cramps were used to complete the union of the stones, and above 24 tons of iron were used for this purpose.

The width of the valley of the *Sarine* at the point where the bridge is built, or, in other words, the distance between the inner face-work of the two gateways on either bank of the river, and consequently the span of the suspended roadway, is 871 feet. It may be easily conceived that a good deal of doubt was entertained as to the propriety of trusting to a span of so great an extent, and the idea of suspending the bridge at the middle at first occurred to M. Challey as the best mode of forming the communication. On weighing the difficulty, however, of obtaining a solid foundation for a pier 220 feet in height in the bottom of an alluvial valley, he soon relinquished this idea; and the bridge has therefore been constructed with a single span of 871 feet.

The roadway is suspended in the manner now universally known, by four *cables of iron wire* * passing over the upper part of the gateways. Each cable consists of 1200 wires, each about $\frac{1}{10}$th inch in diameter, and 1140 feet in length. To avoid the difficulty of moving these heavy cables, each wire was brought separately to its place, and they were united on the spot by the workmen, who were suspended during the work. We are happy to add, that no accident of any kind occurred during this operation. It is calculated that the four united cables are capable of sustaining a weight equal to 2946 tons.

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* It is not perhaps generally known that in all the suspension bridges in France, ropes formed of wires are employed, instead of the solid links used in England.

The four cables are fixed in *chain-pits* or shafts cut out of the solid rock on either side of the river. In each of these pits four cables pass through a vertical cylindric chimney or pillar, which bears three heavy domes resting upon it, and at the same time abutting against grooves cut with much care in the rock to receive the springing stones. At the bottom of the pillars the cables are made fast to blocks of very hard stone which are cubes of 6½ feet. The cables, therefore, cannot slide without lifting the whole of these enormous buildings, strengthened as they are by their connexion with the solid rock.

M. Challey began this work in the spring of 1832. He brought out of France with him only a foreman who had assisted him on former occasions, and engaging in this arduous enterprise with the inexperienced workmen of the country who had never seen a suspension bridge, he completed the work in spite of all these difficulties; and on the 15th October 1834, *fifteen pieces of artillery* drawn by *forty-two horses,* and surrounded by 300 persons, crossed the bridge, though they united in one body as well on the middle as at the ends of the roadway. Nor was the least appearance of derangement of the structure discovered on the closest examination. Some days after the whole inhabitants of Fribourg and its suburbs passed over in procession, so that there were no fewer than 1800 persons on the bridge at the same time; and all classes of travellers, mercantile and curious, have since united with the natives of the Swiss Cantons in testifying their entire satisfaction with the bridge. Although the severe proof to which the constructor of this work subjected it, by loading the roadway with about 20 lb. on each square foot, did not take place till the month of October 1835, yet it may safely be said that the colossal bridge of Fribourg was completely finished in two years and a half. The whole expense was only about L. 24,000.

The only bridge which can be compared for its dimensions with that of M. Challey is the *Menai* or *Bangor* bridge, which joins the Isle of Anglesea to the mainland of England. The largest vessels sail below it with full canvass set. It was constructed by the celebrated engineer Telford; but the *Menai* bridge is only 550 feet in length, while the bridge of Fribourg is 871 feet. The roadway of Mr Telford's bridge is about 106 feet above the level of high water, and M. Challey's 167 feet above the level of the River *Sarine*.

Comparisons with certain points in the city of Paris give a more lively idea than any numbers of the magnitude of the work. Only conceive a bridge of one arch as long as the railing of the Carrousel, or the distance between the wickets leading to the galleries, and a roadway as high as the Towers of Nôtre Dame, or the column in the Place Vendôme, and you may have some idea of the bridge of Fribourg.

On the Volcanic Formations of the Environs of Naples. By M. Dufrenoy.

M. Dufrenoy, in a memoir read to the French Academy of Sciences on the 18th of November 1835, describes successively the deposit of pumice tuff of which the Campania of Naples is composed; and the nature and formation of the hills of the Phlegræan Fields and the group of Vesuvius, in which latter he distinguishes the *Somma* and *Vesuvius* properly so called. He terminates his memoir with various considerations regarding the phenomena which produced the destruction of Herculaneum and Pompeii. In order to give an idea of the labours of M. Dufrenoy, we shall transcribe the conclusions at which he arrives, and which include the principal results of his investigations.

Different Epochs of the Volcanic Phenomena.

1. The igneous phenomena have manifested themselves in the vicinity of Naples at three periods very distant from one another, and with different degrees of intensity, and with different characters.

The *first period*, and of which the geological epoch is unknown, is marked by the deposition of the trachytes, which have afforded the elements of the pumice-tuff; the lavas of the Somma occurring in horizontal masses, and also the leucitic rocks of the environs of Rome.

The production of the trachytes of the Phlegræan Fields and of Ischia took place in the *second period*.

The *third period* includes the lava eruptions of Ischia, Vesuvius, and Monte Nuovo.

Pumice-Tuff.

2. Leaving out of our consideration the volcanic hills, the Campania of Naples, and the islands connected with it, consist of a tuff, composed of the debris of the trachyte of the first period: its elements are almost entirely fragments of pumice, of different sizes. In some circumstances, the fragments are pretty large, but generally, they are so comminuted as to render the tuff argillaceous, and to give it an almost homogeneous aspect. The characters of the tuff, everywhere the same, prove that it owes its origin always to the same cause.

3. This tuff is disposed in thin regular beds, even when they are contorted (Cape of Miceno, Island of Procida, &c.)

It contains fossil shells (Monte Epomeo, Pausilippo, Somma, &c.), and also bones of large animals, viz. of whales, hippopotami, the mammoth, &c. (environs of Rome, coast of Sorrento, Amalfi). This double circumstance proves incontestibly, that this tuff, notwithstanding the height at which it occurs on Monte Epomeo and the Somma, has been deposited under a certain depth of water, in the same manner as the other sedimentary formations.

4. The line of direction of the different hills of the Phlegræan Fields, and the general direction of the "accidens" presented by the stratification of the tuff, both from W. 20° S. to E. 20° N., correspond with the direction of the upraising of the principal chain of the Alps, and this coincidence makes us suppose that the pumice-tuff is contemporaneous, or little posterior to the sub-apennine formations. The nature of the fossils found in this tuff at Monte Epomeo and Pausilippo, confirms the comparison deduced from the study of the directions.

5. The greater number of the minerals which have been collected on the sides of Vesuvius, and which are generally supposed to have been ejected by that volcano, belong to the pumice-tuff. They are contained in the cavities of blocks of saccharoid limestone, or of micaceous rocks having a primitive aspect, which form true pebbles in the midst of this formation. The surface of some of these blocks is covered by Serpulæ, a circumstance which proves that they remained a certain time in the sea before they formed part of the pumice-tuff. Besides,

these blocks are not found exclusively on Vesuvius: we have collected them in the tuff of Pausilippo; and it appears that they exist in the tuff of the Ponza Islands.

Trachytes of the Phlegræan Fields.

6. The hills of the Phlegræan Fields are composed of pumice-tuff; but in the centre of some of them, there are little elevations of trachyte, round which the beds of tuff are arranged. From the relative position of the pumice-tuff and the trachyte in these hills, it appears certain that the latter rock is more modern than the former, and that the *relief* of the Phlegræan Fields was caused by the appearance of the trachyte. The direction of the beds also leads us to think that these trachytes have been produced at the same time that the elevation of the granites of the Alps took place.

7. Monte Epomeo, in the Island of Ischia, also owes its elevation to the trachyte of the second period.

On Vesuvius.

8. Vesuvius is composed of two distinct portions, Somma and Vesuvius. These two parts have been produced by causes of a different order. The Somma forms round Vesuvius a zone of abrupt escarpments, the parallel masses composing which rise on all sides towards the centre: it is the result of a general upraising, which has elevated circularly its component parallel masses, previously horizontal. Vesuvius is the product of partial eruptions and upraisings. The position of the cone of Vesuvius, in the centre of the crater of elevation (soulevement) of the Somma, might make us presume that an intimate connection exists between the two mountains; but they belong to periods separated the one from the other by several great phenomena, which have occurred in the following order:

a, Formation of the lavas of the Somma in horizontal masses.

b, Deposition under the sea of beds of pumice-tuff in horizontal beds.

c, Soulevement or upraising of the Somma at the epoch of the formation of the Phlegræan Fields.

d, Formation of the cone of Vesuvius in the year 79.

9. The difference which exists between the nature and the state of crystallization of the rocks of the Somma and of those of Vesuvius, confirms the conclusions which result from the study of their relative position. The parallel masses of Somma are chiefly composed of leucite and black augite, while those of Vesuvius consist almost exclusively of crystals of the felspar family, probably of anorthite, and of green augite (diopside?).

10. The lavas of Vesuvius always form narrow thin currents, whose texture bears a certain relation to the inclination of the surface on which they have been solidified. They are vesicular and scoriaceous when they have cooled on a surface presenting a higher angle than two degrees, and they then always preserve traces of movement. The lavas, on the contrary, are crystalline and compact, when, having attained a certain thickness on a nearly horizontal surface, they have cooled gradually.

Oscillatory Movements of the Surface of the Campania.

11. The environs of Naples have been subjected to successive depressions and elevations. The temple of Serapis is a celebrated example of these oscillations; and the coast of Puzzuoli affords numerous proofs of these oscillatory movements. There, we see, in almost every part of its whole extent, Roman constructions, covered to a depth of from twenty to twenty-two feet, by sedimentary beds.

Destruction of Pompeii and Herculaneum.

12. The destruction of these two towns does not seem to have been produced exclusively by a shower of cinders; the earthy mass which covers them is in a great measure composed of the same elements as the pumice-tuff which forms the sides of the Somma. We find, besides the fragments of pumice, the same blocks of apparently primitive rocks which contain the minerals said to be derived from Vesuvius. It is therefore probable that the eruption of 79, which ejected a prodigious quantity of cinders, produced also the "eboulement" of a part of the "contreforts" of the Somma, and that from it resulted the ex-

tensive alluvion under which the towns of Herculaneum and Pompeii were buried.

The formation of Vesuvius dates probably from the same epoch, for no tradition and no historical monument anterior to that celebrated catastrophe points in any way to its existence.—*Extracted from the "Compte Rendu" of the Meeting of the Academy of Sciences of the 16th Nov. 1835.*

Description and Drawing of a New Pivot-Castor for Furniture, possessing the advantage of retaining the Oil for an indefinite length of time. By JOHN ROBISON, Esq., F. R. S. E., Vice-Pres. Soc. Arts. *

Read 11th December 1833.

AMONG the various forms given to castors for furniture, none is found to act so well, or to last so long, as that which is commonly called the pivot-castor, or sometimes (when of a large size) the French castor. The objections to its general use are, first its cost, and secondly its depth, which makes it inapplicable in many cases.

Another objection may be applied to it, as well as to all the other forms commonly sold in the shops; namely, that they require constant oiling to prevent them from wearing out of shape, and becoming incapable of turning round.

The castor now about to be described has the advantage of taking no more room than a common castor, while it turns on a fixed pivot, sufficiently long to insure fair action; and containing a reservoir for oil, which will keep it in good order for an indefinite length of time.

These advantages are gained by reversing the places hitherto given to the pivot and the socket. In the pivot castor as usually made, the pivot forms part of the lower portion of the castor, and points upwards; while the socket is in that portion of the castor which is attached to the furniture, and consequently points downwards, which prevents it from retaining more than the film of oil which may adhere to the parts by capillary attraction.

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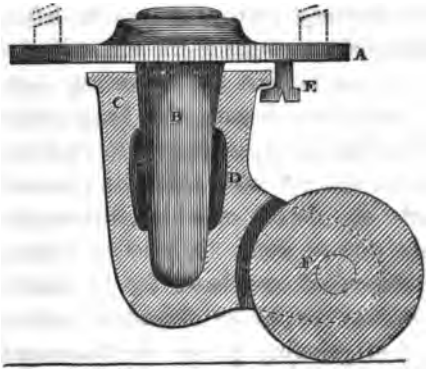
* The Society's Honorary Silver Medal was awarded, 12th August 1835.

In the new castor, the stalk which carries the sheaf is converted into the socket, and the pivot is rivetted to the part which is fixed to the furniture: from this arrangement, the mouth of the socket is upwards, and if a portion of the middle part of it be turned out wider than the pivot, it forms a reservoir in which oil enough for a year or two's supply will find a lodgment.



A, The plate to be screwed to the furniture.
B, The pivot rivetted into A.
C, Section of the socket shewing
D, The space turned out for holding oil.
E, A screwed stud, having the half of its head filed off, so that by unscrewing it half a turn, the socket is allowed to drop off the pivot.
F, The sheave running between shears in the usual way.
A leather washer may be inserted between A and C to keep out dust, but it must not be thick enough to cause friction.

Short Account of the Reverend JOHN FLAMSTEED, *the first Astronomer-Royal.* By FRANCIS BAILY, Esq. Vice-President of the Astronomical Society, &c. &c. &c.[*]

FLAMSTEED was born at Denby, near Derby, on August 19, 1646; and was educated at the free school at Derby, where his father lived. In the summer of 1660, being then about fourteen years old, he caught a violent cold from bathing; the effects of which he felt as long as he lived, and which at this time rendered him so weak, for many years, that he was scarcely able to go to school; and, at length, in May 1662, he finally left it. Being thus withdrawn from school (although not quite sixteen years old) he commenced at that early age a system of study and observation which he pursued unremittingly till the time of his death. In the very same year that he left school, he observed and recorded an eclipse of the sun, a circumstance which shews his early predilection for astronomy; and nearly the whole of his leisure time (leisure, alas, from sickness) was, even in this period of boyhood, employed in mathematical studies and astronomical observations, which he pursued, self-taught, and unassisted; the details of which, written by himself, will be found in the present work. A portion of his time, also, was occupied in mechanical exercises; for he contrived and constructed a quadrant for taking altitudes; and, moreover, employed himself in grinding glasses for telescopes. Flamsteed was naturally of a weak constitution, which was probably increased by the accident just mentioned. His father tried every means of alleviating and removing his complaint; and, finding that the disorder did not yield to medicine, at length assented to his son's request to proceed to Ireland, in order to be *touched* by Mr Valentine Greatrakes, a celebrated empiric of that day, who pretended to cure his patients by a process somewhat similar to the modern practice of *animal magnetism.* He started for Ireland on August 16, 1665; and he appears even then to have had that remarkable habit of noting down in regular order the most minute occurrences and opinions of his life, which he retained to the day of his death; for he has left on record a complete narrative of this journey, detailing a variety of circumstances that occurred on the way. He returned to Derby on September 13, having been absent nearly a month from home.

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[*] This account of the celebrated Flamsteed is extracted from his Life, published from his original manuscripts by Mr Baily. Of this important work 250 copies have been printed by order of the Lords Commissioners of the Admiralty, London, 1835, for *distribution.*

I have been thus minute in these early dates, for a reason which will appear in the sequel, where it will be seen, that they bear materially on a very eventful and critical period of Flamsteed's life. For he is accused by a modern writer (with what appearance of truth, or even probability, the reader will presently have an opportunity of judging) of having committed, about this time, *a highway robbery*, for which he was tried, convicted, and sentenced to be hanged!!! Leaving this subject, however, for the present, and passing over many things that will be found fully detailed in Flamsteed's autobiography, in a subsequent part of this volume, I shall proceed to state, that he pursued his mathematical and astronomical studies at home, and became celebrated in the neighbourhood for his talents; till at length he attracted the notice of several Fellows of the Royal Society in the year 1669; and in the following year he paid a visit to London, where he became acquainted with many scientific persons, but more especially with Sir Jonas Moore, who proved one of his best friends and greatest admirers; and who afterwards (in 1674) proposed to establish him in a private observatory, which he intended to erect at Chelsea College; and, indeed, invited him to London, in order to consult with him on the subject. Whilst in London, he resided at Sir Jonas Moore's house in the Tower, where he carried on his astronomical observations, which are all duly recorded in his manuscript books, and (together with those made at Derby) printed in the first volume of the *Historia Cœlestis.* About this time a circumstance occurred, which induced his Majesty Charles II., to found an observatory at Greenwich; Sir Jonas Moore's proposal of the private observatory at Chelsea was therefore abandoned, and Flamsteed was, through his interest, appointed Astronomer-Royal on March 4, 1674–5. From this period we date the *commencement* of modern astronomy. The invention of the telescope, and the introduction of the clock, then first used for astronomical purposes, were vast improvements on the ancient mode of observing; and their beneficial effects were immediately apparent. Hitherto the catalogue of *Tycho Brahé*, meagre and imperfect as it was, had been the only help and guide to the astronomer for the places of the stars; and the *Rudolphine Tables* (or corrections of the same) for those of the sun, moon, and planets: but Flamsteed resolved to reform and amend the whole system, and he has set a noble example for future astronomers.

Whilst the repairs and fitting up of the observatory were in progress, Flamsteed carried on his observations at the Queen's

House in Greenwich Park, till July 10, 1676, on which day he removed to the Observatory; the only instruments with which he was then furnished, being an iron sextant of seven feet radius, and two clocks, given to him by Sir Jonas Moore, together with a quadrant of three feet radius, and two telescopes, which he had brought with him from Derby; consequently none of these articles were provided at the public expense.

He had not been long in this situation before he was invited, by Dr Bernard of Oxford, to become a candidate for the Savilian professorship of astronomy, then about to be vacated by the Doctor. His reply (February 8, 1697–8), shews the state of his religious feeling at that time, and how far he was satisfied with the situation in which he had been so recently placed; for, in declining the invitation, he says, "I have resolved for the present to content myself with a place which I have furnished with instruments of my own contrivance (*but full of trouble and no gains*), till I see an opportunity of removing to some one more advantageous; and where I may have a better air, with lesser or fewer distempers. *I am as weary of the place as you of yours:* my inclinations are for an employment that may render me more useful in the world, and promote more glory to my Maker; which, as you well intimate, is the *sole end of our lives, and to which I would direct all my labours.*"

In June 1678 he borrowed a quadrant from the Royal Society, which he employed till October 1679, "when the ill-nature of Mr Hooke forced it out of his hands," after which Flamsteed made one of fifty inches radius at his own cost. Finding, however, that he could not determine the equinoctial points, nor pursue his astronomical investigations successfully, without an instrument *fixed in the meridian*, he applied to government from time to time to furnish such a one for the observatory. This was repeatedly promised him, but never carried into effect; and Flamsteed was for some time obliged to make shift with his sextant, brought into the plane of the meridian, and fixed there as well as he was able.* At length, finding all his applications to government fruitless, he resolved to make a *mural arc* at his own expense; the instrument was finished about the end of the year 1681; but, conceiving that it was too slight, and that it was not so accurately made as he could wish, he did not erect it till the year 1683, when he fixed it against the wall, and divided it with his own hands. It proved, however, as he anticipated, to be a failure, and he was obliged to continue his observations with the sextant only, for several years longer.

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* It was about this period, viz. in November 1680, that the great comet appeared; which, after having passed its perihelion, was visible again in the following months. Flamsteed, having investigated its path in the heavens, *immediately* pronounced that the two appearances were *one* and the same comet;

During all this time (a period of nearly fifteen years), government had not furnished him with a single instrument. It is true they had given him a house to live in, and had appropriated a *precarious* salary of L. 100 a-year;* but, at the same time, although his employments were sufficiently laborious, the King had ordered that he should instruct, monthly, two boys from Christ Church Hospital, which was a great annoyance to him, and interfered with his proper avocations. The government had, however, provided him with "a surely silly labourer" to assist him at the sextant; but another assistant was necessary for the ordinary work of the observatory, and Flamsteed was obliged to provide such additional help at his own charge; for it was not in those days as at the present times, when the astronomer-royal is not only provided with a competent salary, but with all the requisite instruments and assistants likewise; and when all the comforts and conveniences for carrying on an extensive and regular system of observations, and for reducing the same, are furnished at the public expense. In order to meet these and other charges which Flamsteed had incurred in carrying on his observations, and which he could ill afford, he entered on the laborious task of a *teacher*, by which, it is true, he derived a scanty addition to his means, but was at the same time unavoidably drawn away from the main object of his appointment. With such miserable shifts and such obstructions as these, he was obliged continually to struggle, so that his progress was ne-

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whilst Newton for a long time maintained that they were *two* separate comets. Before the *Principia* was published Newton had discovered his error, and in that work acknowledges that Flamsteed was right.—See p. 50.

* See his letter to Sir Jonas Moore, No. 9, and to the Bishop of Salisbury, No. 10. in the appendix. In the former of these he says, "I cannot conceive that you have any real design to stop my salary, which I have earned by labour *harder than thrashing*."

cessarily slow, and he could not make much advancement in the fundamental points of astronomy. It is true that he observed an immense number of intermutual distances of the stars with the sextant, but he was obliged to depend on Tycho's catalogue, for their positions with respect to the equinoctial points, having no instrument for determining such quantities.

When this first mural arc was finished, Flamsteed found, as I have already stated, that it was made too weak for his purpose; nevertheless he contrived to take with it the meridional altitudes of a great number of stars; by means of which, and the intermutual distances taken with the sextant, he formed an *approximate* catalogue of a few of the principal stars to serve his present purpose. The reader is requested to bear this circumstance in mind, as it explains and justifies a part of the conduct pursued by Flamsteed towards Newton, as related in the subsequent pages. Yet, notwithstanding all these difficulties under which Flamsteed laboured, notwithstanding the obstructions thus thrown in his way, the public (the scientific public of that day, not the ignorant and unwary multitude, for they knew nothing of the matter), were repeatedly asking "why he did not print his observations?" * Flamsteed replied very justly, that he had as yet made no observations that could be turned to any valuable account, for want of the requisite instruments; indeed, it could scarcely be expected of him that he should be able to make "bricks without straw."

About this period (1684), he was presented to the living of Burstow by the Lord Keeper North; soon after which his father died (1688); and Flamsteed, finding his income somewhat increased by these events, resolved on expending a portion of his property in constructing a new mural arc, much stronger than the former. He had been assured by Lord Dartmouth, the Master of the Ordnance, that whatever he laid out on this occasion should be repaid to him; but in this also he found himself, eventually, most grievously disappointed, as he never received a farthing for the moneys expended on this instrument, which cost him upwards of L. 120. The instrument here alluded to is the celebrated mural arc made and divided by Mr Abraham Sharp, with which Flamsteed subsequently made all those observations from which the *British Catalogue* is deduced. From this moment (September 1689, when the instrument was first used), every thing which Flamsteed did, every observation that he made, assumed a tangible and a permanent form, and was available to some useful purpose; his preceding observations being only subsidiary, and dependent on results to be afterwards deduced from some fixed instrument of this kind, which he had long sought for. It was at this point only that the observatory could be considered complete; and from this period we must date the commencement of his valuable and fundamental observations.* In reading the subsequent history of Flamsteed's life, it is necessary to attend to these several divisions of his labours.

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* "Some people," says Flamsteed, "to make me uneasy, others out of a sincere desire to see the happy progress of my studies, not understanding *amid what hard circumstances I lived*, called hard upon me to print my observations."—See p 54.

The observatory had now been established upwards of fourteen years; it remained under Flamsteed's superintendence upwards of thirty years more (being nearly half a century from his first appointment of Astronomer-Royal); nevertheless during this long interval the government had not furnished it with a single instrument, nor had they allowed him the cost of a single computer to reduce his observations. Even those which were lent to him by the Royal Society were taken away from him as soon as his patron, Sir Jonas Moore, died.

The *whole* of the instruments were Flamsteed's own, the government not having even been at the expense of *repairing* them; and the *whole* of the observations had been reduced at Flamsteed's own charge (many of them in duplicate), and arranged by him into catalogues and tables. Yet *(proh pudor!)* in the latter portion of his life, as we shall presently see, the differences from the places observed, at that time a most valuable document; copies of which he gave to Sir Isaac, for his private use to rectify the lunar theory; on this express condition, however, that he should not impart them (or the results obtained therefrom) to any person without Flamsteed's consent, for this obvious and just cause, that the places of the moon were determined only by means of his *approximate* catalogue above mentioned.* This interview led to a correspondence between them relative to this and other astronomical subjects, the major part of which has never before been made public.† In the spring of 1696, Newton was made Warden of the Mint, and came then to reside in London; where Flamsteed says, that he sometimes visited him in Jermyn Street; that they continued civil towards each other, but that Newton was not so friendly as formerly. Here, then, we trace the first symptoms of that coolness between them which soon afterwards broke out into an open rupture, the immediate cause of which appears to be as follows.

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* I do not wish to be considered as hereby intending to depreciate Flamsteed's previous labours with the sextant, and which are printed in the first volume of his Historia Cœlestis; on the contrary, I consider those observations as equally correct with those made with the mural arc, and as available, in many instances, in determining the relative positions of the fixed stars; though not so frequently appealed to, on account of the trouble required in computing the results. They had, however, all been reduced by Flamsteed, and many of the results compared with those obtained from the mural arc.

Dr Wallis having understood that Flamsteed had written a paper "On the Parallax of the Earth's Annual Orb," requested a copy of it, for the purpose of its being published in the third volume of his Mathematical Tracts, then in the press.‡ Flamsteed accordingly furnished him with a copy of it in English, which Dr Wallis translated into Latin.* It appears that there was (in the original) the following paragraph alluding to his having furnished Newton with several observations of the moon, as above mentioned, viz. "Contraxeram etiam cum D° Newtono, doctissimo tunc temporis in Academiæ Cantabrigiensi Professore, necessitudinem, cui lunæ loca ab observationibus meis ante habitis deducta 150 dederam, cum locis simul e tabulis meis ad earum tempora supputatis, tum similium in posteriore prout assequerer promissorum, cum elementis calculi mei, in ordine ad emendationem theoriæ lunaris Horroccianæ." At which Newton (on hearing of the circumstances through the officiousness of Dr Gregory), was very indignant, and wrote that most extraordinary letter to Flamsteed, dated January 6, 1698–9, which is inserted in the appendix, No. 43: "I do not love (says Newton) to be printed upon every occasion, much less to be dunned and teazed by foreigners about mathematical things; or to be thought by our own people to be *trifling* away my time about them, when I should be about the king's business. . . . . You may let the world know, if you please, how well you are stored with observations of all sorts, and what calculations you have made towards rectifying the theories of the heavenly motions; but there may be cases wherein your friends should not be published without their leave, and therefore I hope you will so order the matter that I may not, on this occasion, be brought upon the stage."† There is surely nothing in Flamsteed's let-

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* This request was not only reasonable but *mutual;* for Newton frequently enjoined the same restrictions upon Flamsteed. In one of his letters (No. 25. in the Appendix) he proposes to send Flamsteed a new table for the moon, on the express condition that he shall keep it to himself till Newton has perfected the lunar theory, because it would need correction; and that Newton acknowledged Flamsteed's claim, is evident from a letter which he wrote about the same period (No. 26. in the Appendix), wherein Newton says, "I only assure you at present that, without your consent, I will neither publish them nor communicate them to any body whilst you live, nor after your death, without an honourable acknowledgment of their author."

† These letters are now given in the Appendix, No. 16–34. Some of Newton's letters, more especially Nos. 30. and 31. do not seem to have been written in a very courteous style. Indeed, Flamsteed has remarked that Newton's *conversation* was not always of the most engaging kind, since he was sometimes so presumptuous as to ask him "why he did not hold his tongue."—(See page 73.)

‡ This is the celebrated letter of Dr Wallis, in which Flamsteed clearly points out the effect of *Aberration;* and indeed defines its amount, which accords remarkably well with modern observations. A similar effect had been noticed, may years previous thereto, both by Hooke and by Picard, almost

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immediately after the application of the telescope to astronomical instruments; and in fact it was a necessary consequence of that invention. Flamsteed, however, as well as his predecessors, mistook the cause, which they attributed to the *Parallax of the Earth's Orbit*; and it was reserved for Bradley to develop and explain the true theory of the phenomenon, and its application to the purposes of astronomy.

* At least, so it is distinctly stated by Wallis and Flamsteed; but if we judge from the specimen contained in the letter which Wallis wrote to Newton, mentioned in the text, and which is given at full length in the *addenda*, we can scarcely imagine the Latin to have been composed by Wallis himself.

† Sir David Brewster (in his recent Life of Newton, page 243) has, through some singular error or confusion, attributed this letter to Flamsteed instead of Newton; stating at the same time (I know not upon what authority), that it is "characteristic of Flamsteed's manner;" and thence draws the conclusion that Flamsteed, not sufficiently aware of the importance of the inquiry, re-

ter which should warrant expressions of this kind from Newton; and Flamsteed's reply to him (see p. 168), was written in a very different style. "I would not think (says he) you would be unwilling our nation should have the honour of furnishing you with so many, and good, observations for this work (the lunar theory) as were not (I speak it without boasting) to be had elsewhere. . . . I thought not that it could be any diminution to you, since you pretend not to be an observer yourself.* . . . You will pardon me this freedom, and excuse me when I tell you, if foreigners come and trouble you it is not my fault, but those who think to recommend themselves to you, by advancing the fame of your works as much as they possibly can. . . . I wonder that hints should drop from your pen, as if you looked on my business as *trifling*; you thought it not so, surely, when you resided at Cambridge; its property is not altered. . . . The works of the Eternal Providence, I hope, will be a little better understood, through your labours and mine, than they were formerly. Think me not proud for this expression; I look on pride as the worst of sins; humility as the greatest virtue. This makes me excuse small faults in all mankind, bear great injuries without resentment, and resolve to maintain a real friendship with ingenious men, to assist them what lies in my power, without the regard of any interest, but that of doing good by obliging them." Flamsteed immediately wrote also to Dr Wallis to request him to withdraw the harmless but offensive paragraph.†

This short but unexpected correspondence appears to have terminated all amicable relations between Newton and Flamsteed; and from this period we must consider their friendship

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ceived Newton's requests as if they were idle intrusions, in which the interests of science were but slightly concerned. This inference, however, now falls to the ground, and the erroneous impression cannot be too speedily removed. The history of the whole affair will be found in the Appendix, No. 35, 46.

* Newton himself confesses this, in his letter inserted in page 151, where he says, "All the world knows I make no observations myself, and therefore I must of necessity acknowledge their author; and if I do not make a handsome acknowledgment, they will reckon me an ungrateful clown."

† Dr Wallis likewise wrote to Newton on the subject already stated; and in my late visit at the Earl of Portsmouth's, to inspect the Newton MSS., I found the original letter, from which the above mentioned paragraph in page xxxiii is extracted, and which is inserted in the *addenda*.

at an end, although the outward forms of civility were still kept up. The reader, however, may be somewhat surprised to learn that, not more than a month previous to this time (namely on December 4, 1698), Newton had paid a visit to the Observatory, late in the evening, for the express purpose of procuring twelve more computed places of the moon, which he had previously requested from Flamsteed, for some special purpose in his investigations.* And in order to understand the value and importance of these favours, it should be constantly borne in mind, that there was no other source in this country (nor on the Continent, as far as I can learn) from which such information could be obtained. The Paris observatory had been established ever since the year 1671; but hitherto only detached observations had been published.

Flamsteed continued for several years to pursue his observations, as well as his health and circumstances would permit; and in the course of that time, had not only formed a catalogue of two or three thousand stars, whose position he had determined with his new mural arc, but had also suggested several corrections to the solar, lunar, and planetary tables, which he was by such means enabled to supply. He now began to entertain serious intentions of publishing the result of his labours, and wrote an estimate of the number of printed sheets it would fill. He had already expended upwards of L.2000 in furnishing instruments for the

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* In page 65, the reader will find the entry which Flamsteed made in his Observation Book, relative to this visit of Newton: but I have since discovered the following entry of the same event, in MSS. vol. xvi., which is rather more minute. "Decem. 4. die ⊙ post preces vespertinas visum me veniens Ds. Is. Newtonus, Cantabrigiæ Matheseos Professor, &c., ascent. rectas ☽ cum distantiis a polo, ab observationibus computo deductas 12, e pag. 184 et 185. libri 5. calculationum transcriptas quas petiit communicatas habuit." And on referring to the said 5th book of calculations (MSS. vol. lv.), I find in page 181 a memorandum, that Newton had requested to have the computed places of the moon for the following days, viz. June 22, 1694, April 25, May 13, 24, and 26, June 11, 16, 25, and 27, July 7, 9, 11, and 15, and August 8, 1695; all of which (except those of June 27 and August 8) are calculated by Mr Hodgson in page 183, and copies of them were forwarded to Newton. Flamsteed, however, discovered soon after that these computations were erroneous, and has himself calculated them anew on pages 184 and 185, as above mentioned. And it was to obtain these twelve corrected values, that Newton paid this visit to the Observatory. Yet within a month after this event, as I have just stated, he wrote Flamsteed that most extraordinary letter.

Observatory, and in hiring assistants and computers, all of which ought, in fact, to have been defrayed by the Government, from whom, however, during this long period, he had never received a single farthing beyond his scanty salary. Although by no means a mercenary man, he might have indulged a hope of being enabled to get a return for some portion of this outlay, by means of subscribers to his work; but this specific plan was in some measure obviated by the interference of Prince George of Denmark, who, towards the end of the year 1704, having heard of these extraordinary labours of Flamsteed, and being himself a patron of science, proposed to print the observations and the catalogue at his own expense. A committee, consisting of Sir Isaac Newton (then president of the Royal Society), Sir Christopher Wren, Dr Arbuthnott, Dr Gregory, and Mr Roberts, was appointed to inspect the papers, who reported favourably upon them, and recommended them *all* to be printed.* The publication of the work was therefore placed under their superintendence; and Flamsteed, who did not anticipate much benefit from Newton's interference, thus found himself unwarily involved in fresh troubles and contentions. For the *referees*, as this committee was called, or rather Sir Isaac Newton (for he appears to have assumed the principal management of the affair), seem to have conducted the business without Flamsteed's privity or concurrence, and, notwithstanding Flamsteed's repeated remonstrances, to have thrown every obstacle in the way of dispatch; at least, this is Flamsteed's version of the matter, and his view of it appears to be confirmed by the documents in the appendix. Sir Isaac pretended to have discovered several errors, and demanded the books containing the original entries, in order that he might compare and examine them. Having got these into his possession, he next required that that portion of the catalogue which was completed (but which was not to be sent to press till after the whole of the observations were printed, so as to allow time for its being perfected) should be placed, sealed up, in his hands. Flamsteed at first resisted: he told Sir Isaac that the catalogue was not complete; that it would eventually contain a great many more stars than he had yet observed and rectified; that it at present contained only about 1500, but that he hoped to make it up to 2500 stars; that these were the result of all his labours, in which he had spent above L.2000 more than his salary; and that it would not be either prudent or safe to trust a copy of it out of his own keeping. He at length, however, found himself obliged to comply, or else to give up the prospect and advantage of having the work printed at the Prince's expense; and the catalogue, imperfect and incomplete as it was, was accordingly sealed up in the presence of Sir Christopher Wren, and delivered into Sir Isaac Newton's possession. New difficulties, however, were afterwards started, oftentimes frivolous and vexatious, and it was May 16. 1706, before the first sheet was struck off; and it was Christmas 1707 (three years after the first undertaking) ere the whole of the *first* volume only was finished; during which time the press was frequently stopped by Sir Isaac, without any assignable cause. The whole details of these proceedings are given by Flamsteed in the following history of his own life, and supported by various documents which are inserted in the appendix.

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* Although the referees here recommend that the whole of the observations should be printed, yet we shall find in the sequel, that their opinion upon this subject experienced some alterations, at least, if we may judge by the

This *first* volume, which contained only his sextant observations, being thus completed, arrangements were entered into for proceeding with the *second* volume, which was intended to contain the observations made with the mural arc. After a great deal of unnecessary procrastination on the part of Sir Isaac Newton, a meeting with the referees was appointed to take place on March 20, 1707–8, when Flamsteed took up with him the whole of the observations made with the mural arc, from September 1689 to December 1705, fairly copied out on 175 sheets of large paper, together with a more extensive and perfect copy of his catalogue of the fixed stars. At this meeting new articles were suggested, and finally imposed upon Flamsteed; for he was not only obliged to leave the whole of the 175 sheets of manuscript in Newton's hands, but also bound himself to complete, and return within sixteen days, the catalogue which had previously been delivered, sealed up, to him; Sir Isaac retaining the one which Flamsteed had brought with him, as a pledge for the performance of the contract.* Notwithstanding this compliance, however, on the part of Flamsteed, the work of the press does not seem to have been expedited; further obstructions were thrown in the way of proceeding, the nature and cause of which are not sufficiently apparent; and Prince George died (October 28, 1708) before the second volume was entered upon. The work was now completely stopped; and although by this melancholy event the power of the referees ceased, the papers were still left in their hands.

Being now undisturbed (as Flamsteed expresses himself), he proceeded to carry on such observations as he wanted for the purpose of his astronomical inquiries, and added many new stars to his catalogue. Nothing more was heard about Sir Isaac Newton or the printing; and Flamsteed says, in one of his letters to Mr Sharp, "I shall not urge it forward again, till I see a good fund settled and secured for carrying it on, without any danger of impediment or obstruction from him or any of his tools." But in the midst of this apparent quiet, he was again annoyed, when he least expected it, by being privately informed that his catalogue (which he had delivered, sealed up, into Sir Isaac Newton's hands, as a sacred deposit) *was in the press*; but more so, by a letter from Dr Arbuthnott (dated March 14, 1710–11), demanding the deficient parts of such catalogue, and informing him that he (Dr Arbuthnott) was commanded by the Queen to superintend and complete the publication of the *Historia Cœlestis*, undertaken by the late Prince. Dr Arbuthnott, however, appears to have put the business into the hands of the Royal Society, who thus became in some measure mixed up with the subsequent proceedings; but Newton and Halley were evidently the prime movers on every occasion. Halley was (I believe) at that time clerk to the Society.† Flamsteed was much annoyed at this new step: he requested and obtained an interview with Dr Arbuthnott, and at the conference that ensued (March 29), he asked the Doctor in direct terms, "whether the catalogue was printed or not?" to which the Doctor replied, "that not a sheet of it was printed." Flamsteed doubted the assertion at the time, and which, indeed, turned out to be *false*; for a friend sent him, within four days after, the constellation of *Aries* and *Taurus* fairly printed; and, in a day or two after, that of *Virgo*. He learned also that Halley had the superintendence of the press; that he pretended that he had found many faults in the catalogue; that he had, moreover, shewed some sheets of it publicly at Child's Coffee-house, and that he boasted of the pains he had taken in correcting the errors. Flamsteed was of too high a spirit to be thus treated, without remonstrance: he found that he had been made the dupe of some intrigue, and he resented it accordingly. In one of his letters to Dr Arbuthnott (April 19, 1711), complaining, amongst other things, of the alteration in his catalogue, he says, "I have now spent thirty-five years in the composing and work of my catalogue, which may in time be published for the use of her Majesty's subjects, and ingenious men all the world over. I have endured long and painful distempers by my night watches and day labours. I have spent a large sum of money, above my appointment, out of my own estate, to complete my catalogue, and finish my astronomical works under my hands. Do not tease me with banter, by telling me that these alterations are made *to please me*, when you are sensible nothing can be more displeasing nor injurious than to be told so. Make my case your own, and tell me, ingenuously and sincerely, were you in my circumstances, and had been at all my labour, charge, and trouble, would you like to have your labours surreptitiously forced out of your hands, conveyed into the hands of your declared, profligate enemies, printed without your consent, and spoiled, as mine are, in the impression? Would you suffer your enemies to make themselves judges of what they really understand not? Would you not withdraw your copy out of their hands, trust no more in theirs, and publish your own works rather at your own expense, than see them spoiled, and yourself laughed at for suffering it?

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* This continual suspicion appears to me to have been exerted on the wrong side; for it was Flamsteed that had most reason to be cautious, since *he* would have been the only sufferer by any breach of the agreement.

† I shall still call these parties *referees*, for want of a better designation; for although the original committee was dissolved, yet it is evident that the same *animus* existed in those who formed the new body of advisers.

"I see no way to prevent the evil consequences of Dr Halley's conduct but this. I have caused my servant to take a new copy of my catalogue, of which I shall cause as much to be printed off as Dr Halley has spoiled, and take care of the correction of the press myself, provided you will allow me the naming of the printer, and that all the last proof-sheets may be sent to Greenwich at my charge by the penny-post, and not printed off till I have seen a proof without faults. After which I will proceed to print the remaining part of the catalogue as fast as my health and the small help I have will suffer me. But, if you like not this, I shall print it alone at *my own charge* on better paper, and with fairer types than those your present printer uses; for I cannot bear to see my own labours thus spoiled to the dishonour of the nation, Queen, and people. If Dr Halley proceed, it will be a reflection on the President of the Royal Society, and yourself will suffer in your reputation for encouraging one of whom the wisest of his companions used to say, that the only *way to have any business spoiled effectually was to trust it to his management.* But I hope better things of you, and that you will endeavour to make me easy after all my long, painful, and chargeable labours, by affording me your assistance as occasion shall serve; whereby you will ever oblige, Sir, your humble servant and sincere friend."

This remonstrance being of no avail, it appears that Flamsteed addressed the Queen upon the subject, for there is amongst his MSS. the copy of a petition, dated April 16, 1712, stating the circumstances of the case, and requesting that this surreptitious edition of his catalogue might be suppressed. Flamsteed, however, remonstrated here likewise in vain; for he found soon after not only that the printing of the spurious catalogue was completed, but also that the observations made with the mural arc (contained in the 175 sheets which were left in the hands of the referees as above mentioned) were sent to the press *in a garbled and incorrect manner*, the observations of those stars *only* being retained which passed the meridian at the same time with the moon and planets, and nearly on the same parallel, *the rest being wholly rejected* *. He also found that the places of the moon inserted in the margin of the book, and considered to be deduced from those observations, were the very same places (at least those in the more early periods) that he had some years before given to Newton under the express stipulation that they were not to be made public, because they were deduced from an *approximate* catalogue of the fixed stars. This was not just either to Flamsteed or to the public, who had a right to expect that the most correct determinations should be given *. It is true that the editor thereby saved himself a vast deal of intricate and troublesome computation; but the character of Flamsteed suffered in proportion, and we cannot be surprised that he should be indignant on the occasion. And, if he has expressed his opinion of Halley's conduct (in his confidential letters) in terms which sound at the present day extremely harsh to our ears, it must be confessed he had much to irritate and excite him.

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* In order that the reader may fully understand the nature of this change, (which is by no means a light one, and of which Flamsteed might justly complain), it may be proper here to state, that the edition above alluded to does not contain the journal of the observations made with the mural arc, in the man-

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ner in which they were entered in the MSS. books, (and as they are in fact now printed by Flamsteed in the *second* volume of the *Historia Cœlestis*, but merely *partial extracts* from the same, where they had reference to the moon or any of the planets, *all the remaining observations being wholly omitted.* And these extracts were arranged under different heads, according to the body with which the stars (generally two or three only in number) were compared. Thus, on 15th September 1690, although there were 119 observations made, yet only the five which relate to Jupiter, and the four which relate to the moon, are extracted for the press, and placed in *different parts* of the volume; the remaining 110 observations being wholly *omitted*, and no notice whatever taken of them in any part of the book. So that the future astronomer has no means of correcting the error of the instrument or of the clock, nor of ascertaining whether the catalogue of the fixed stars had been correctly deduced. (See the last note in page 92.) Flamsteed knew much better than the referees the practical advantage of having *all* the observations recorded day after day *in their regular order*. He was therefore perfectly justified in destroying (as he afterwards did) this garbled and abortive production, and both the present and future astronomer will duly estimate the obligations which they are under to him for having had the public spirit afterwards to print at his own expense the whole of his observations in the order in which they were made. Flamsteed's motive, however, was but little understood in his day, if we may judge from the opinion of Mr Jones alluded to in page 20.

* The *early* computations of the places of the moon are to be found in the MSS. vol 54, and correspond exactly with those published in Halley's spurious edition. The subsequent lunar computations, deduced from the *correct* places of the stars, are to be found in MSS. vol. 60, and correspond with those published afterwards by Flamsteed himself. The difference is frequently very considerable. See Mr Sharp's opinion on this subject in page 323.

Flamsteed, however, had not sufficient interest to stop the press, for the work, thus *mutilated and corrupted*, ultimately appeared in one volume, accompanied with a disingenuous and illiberal preface by Halley, who superintended the edition [*]. This conduct of the referees was evidently unjustifiable, as they had no right to break the seals of his deposit without his consent and approbation, even at the command (as they pretend) of the Queen [†]. The whole of the documents were clearly Flamsteed's own; the observations had been made with his own instruments, and reduced at his own expense; the Government had not (as I have repeatedly remarked) contributed any thing beyond his paltry salary of L. 100, and that charged with the execution of duties that belonged not to his situation. The least, therefore, which they could have done, should have been to let him print his own works in his own way, not only on account of the labour, the anxiety, the money which they had cost him, but also, and more especially, because there was no one so competent as himself to judge of the most proper manner in which they ought to appear before the public for the promotion of astronomy. The *whole* would then have been finished in much less time than this single volume of Halley's.

This spurious and premature publication of his works was a mortifying circumstance to Flamsteed, and annoyed him very much; and it cannot be wondered at that he should so feel it, and resent it accordingly. In his correspondence with Mr Sharp on this subject, he opens his whole mind on the subject, calls Halley

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[*] This edition will frequently be referred to in the subsequent pages, as "Halley's editions of 1712." It contains, besides the spurious *Catalogue* and the garbled *Observations*, nearly the whole of what now forms the first volume of the *Historia Cœlestis*. In the preface Halley has made many representations and misstatements, some of these I have pointed out in pages 385 and 386; and I will here farther add, in contradiction to what Halley has stated, that *it was not* agreed that the catalogue should be prefixed to the first volume, and that he has in many other parts of the said preface given a colouring to facts which leave a false and erroneous impression on the mind of the reader. There are very few copies of this work now in existence, nearly the whole of the edition having been destroyed by Flamsteed, as will be related in the sequel.

[†] Flamsteed says that the order of the Queen was obtained *after* the offence was committed. This is a question, however, of but little moment in a case of absolute wrong.

"a malicious thief," and makes use of other opprobrious epithets which could only be palliated by a consideration of Flamsteed's high state of excitement. But, I apprehend, that, at that day, a much greater license of expression was allowed or taken on such occasions; for a circumstance occurred about the same time, which shewed that even Newton himself could for a moment, in a similar manner, forget his rank and station, the occasion of which was as follows:—In the year 1710, her Majesty was pleased to appoint the President of the Royal Society, together with such others as the Council of the said Society should think fit, to be *visitors of the Royal Observatory.* Flamsteed calls this measure "another piece of Sir Isaac Newton's ingenuity;" and, after the treatment he had received, he might naturally conclude that this also was done to annoy him. There is no evidence, however, to shew that Newton had any hand in it whatever; but, in consequence of this appointment, a scene occurred, the particulars of which would perhaps never have been divulged, had not these manuscripts of Flamsteed, belonging to two distinct parties, been simultaneously brought to light. It appears that a meeting of the council of the Royal Society was summoned for October 26, 1711, at which Flamsteed was desired to attend, "to know from him if his instruments were in order, and fit to carry on the necessary celestial observations." Flamsteed attended accordingly; and a scene ensued, which he has minutely described in three or four of his MSS., and in his letters to Mr Sharp without much shade of difference. It appears that Newton, not satisfied or pleased with the answers that he received from Flamsteed, forgot himself and the duty he was then performing under the Queen's warrant, "ran himself into a great heat and very indecent passion, and used him so as he was never used before; called him a *puppy*, and many other hard names, but *puppy* was the most innocent of them." Dr Mead, who was present, joined in insulting him, till at length Flamsteed, evidently disgusted at such treatment, withdrew from the scene, desiring them to restrain their passion, and telling them that "it was a dishonour to the nation, her Majesty, and that Society (nay to the President himself) to use him so." When we consider that Newton was at that time nearly sixty-nine years of age, and that Flamsteed was upwards of sixty-five, and so infirm that he was obliged to be assisted both up and down stairs, it must be confessed that this scene exhibits but a miserable picture of the frailties of human nature, and every friend to science, or even to humanity, must lament its ever having taken place.

Soon after this occurrence, it appears that Flamsteed, finding that all faith with him had been broken, that his catalogue had been thus surreptitiously and clandestinely printed, and that his observations also had been sent to the press in a garbled and improper manner, broke off all communication with Dr Arbuthnott, and his coadjutors in this affair, resolving in his own mind to appeal to the public on the occasion. He drew up a statement of all the proceedings that had taken place, with a view to its publication; and afterwards set about a re-examination of his observations, in order to collect together, for insertion in his catalogue, such stars as had escaped his notice in his former reviews, determined to perfect the catalogue as much as possible, and to reprint it at his own expense; and before the end of the year 1712, he received the last sheet from the press. He then proceeded to do the same with his Observations; and, for this purpose, he applied to Sir Isaac Newton for the manuscript copy not only of the catalogue, and of the 175 MS. sheets of Observations which had been deposited in his hands, but also of the MS. books of *original entries*, which had been left with him some time before, but without effect. Flamsteed, therefore, found himself obliged to commence legal proceedings against him for their recovery, but with what success I have not been able to ascertain. Some of the books were returned to Flamsteed, but there is still one of them missing (containing the MS. observations from November 1702 to January 1712), which perhaps is the one that Flamsteed denies ever having received back. With respect to the 175 sheets of MS. observations, it appears that Newton eventually handed them over to Halley; which Flamsteed calls "the height of trick, ingratitude, and baseness." And it is certain that Flamsteed was ultimately obliged to recopy not only the Catalogue, but also these 175 sheets of observations, for the press, at an expense of nearly L. 200, and at a great loss of time and labour, independent of the additional risk of error. This conduct was indeed unaccountable, and scarcely to be justified on any view of the case.

Whilst employed, however, on this work, two events occurred, which in some measure changed the prospect of Flamsteed's affairs. These were the death of Queen Anne, who died on August 1, 1714, and the death of the Earl of Halifax, the great patron and supporter of Sir Isaac Newton, on May 19, 1715. The officers at court were changed: the new Lord Chamberlain knew Flamsteed well; and a hint was given to him that he might, with very little trouble, get all the spurious copies of his printed observations into his own hands. He accordingly drew up a memorial and petition to the Lords of the Treasury (Sir Robert Walpole being the First Lord); whereupon 300 copies of this obnoxious work, probably all that remained out of the 400 printed, after the presentation copies and a few sales were deducted, were delivered up to him, *which he immediately committed to the flames*, "that none might remain to shew the ingratitude of two of his countrymen, who had used him worse than even the noble Tycho was used in Denmark." Rejoiced at this circumstance, he set himself in earnest to print his observations in the order in which they were made, and as they now appear in the second volume of the *Historia Cœlestis;* for, though, as he candidly states, "he was unwilling to impoverish his nearest relations, whom he was bound in justice and conscience to take care of, since they were in no capacity to provide for themselves," yet he was determined that the labour of nearly forty years should not be thrown away, and therefore resolved to print them *at his own expense.* Fortunate, indeed, has it been for the astronomer that Flamsteed was so resolute and pertinacious on this point; and that he had courage and public spirit enough to bear up against his two powerful opponents, whose views upon this subject are by no means in accordance with those of modern astronomers.

It should here be remarked, that when Flamsteed obtained the 300 copies of his printed work from the Lords of the Treasury, he destroyed only the catalogue and the spurious part of the work which professed to be his observations made with the mural arc. That portion of it which contained his observations with the sextant was separated from the rest, and (together with the observations of Gascoigne and Crabtree, and of his own at Derby, as well as the computed places of the moon and planets, and a few subsidiary tables, all printed afterwards at Flamsteed's own charge), now forms the first volume of the *Historia Cœlestis*. So that, of all the three volumes of the *Historia Cœlestis*, there were only ninety-seven sheets, of this first volume, that were printed at the public expense, all the rest having been edited at the risk and private cost of Flamsteed himself.

Flamsteed, however, did not live to see the termination of his labours: he died before the second volume was quite completed; and the remainder of that volume, as well as the whole of the third, was finished under the care and superintendence of Mr Joseph Crosthwait, his assistant at the Royal Observatory, aided by Mr Abraham Sharp. In the complete and perfect execution of this undertaking, they met with many difficulties; for although Mrs Flamsteed appears to have been a woman of high spirit, and impressed with a proper sense of, and regard for, her husband's honour and fame, yet a too strict attention to economy prevented the work from appearing before the public in the most advantageous light. The catalogue, which had been reprinted by Flamsteed, was still found, on a new comparison with the observations by Mr Crosthwait, to contain many errors; some of the sheets were again reprinted with amendments, but others were suffered to be ultimately published with all their faults. Yet, had it not been for Mr Crosthwait's extraordinary, and in some measure gratuitous exertions, the work would never have been completed; and the world must have been satisfied with the meagre and garbled edition published by Halley.* The Preface cost Mr Crosthwait much trouble; it was written in English by Flamsteed, but it was now required to be translated into Latin; no one, however, could for some time be found adequate to the task, though repeatedly attempted. Mr Pound at one time undertook it, but eventually, after much procrastination, declined it; and it was at last accomplished by a dissenting minister; a considerable portion of it, however, being suppressed, as already mentioned. The whole work was at length published in three volumes in 1725, six years after Flamsteed's death. The distribution of its several parts will stand thus: the first volume, and the major part of the second volume, were printed during Flamsteed's lifetime; but the remainder of the second and the whole of the third volume, were printed under the superintendence of Mr Crosthwait. This latter portion therefore may, in some measure, be considered as a *posthumous* work.

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* Mr Crosthwait had a great esteem and veneration for Flamsteed. He attended him in his last illness, on his deathbed; and, in the account which he gives of that scene to Mr Sharp, he says, "He often called for me, and would gladly have said something to me, but was not able, though he called for me by name, and continued to do so till the last moment. You will see by this, that he has not left me in a capacity to serve him, notwithstanding he has often told me he would; but this I impute to his not being sensible of his near approach till it was too late; but the love, honour, and esteem I have (and shall always) for his memory, and every thing that belongs to him, will not permit me to leave Greenwich or London, before, I hope, the three volumes are printed."—(See page 333.) And in another letter he states, 'Had it not been for the love and honour I bear to Mr Flamsteed's memory knowing how many potent enemies he has left behind, and how few friends

There remained now only the *maps*, the construction and engraving of which appear to have cost as much trouble and vexation as the letter-press, but arising from a totally different source. It seems that only one of them was completely finished (Orion[*]) when Flamsteed died; for the rest we are indebted to Mr Sharp, who constructed them anew, according to Flamsteed's principles, from the catalogue. Sir James Thornhill drew the figures of the constellations, and recommended engravers for the work; but the charges of the English artists were considered so enormous, that Mr Crosthwait went over to Holland for the express purpose of engaging some of the best Dutch engravers to complete the work. The vexatious delays which necessarily occurred by adopting this method, its increased expense, and the constant attention requisite to prevent mistakes, dispirited Mrs Flamsteed; and a temporary stop was consequently put to the work, although Mr Sharp (now much advanced in years) and Mr Crosthwait were willing to continue their services. At length some English engravers being found who offered to execute the maps at a more moderate charge, the labours of these gentle-

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capable of serving him in these affairs), I had before this time left Greenwich, and should have had a due regard to my own future support; but this I have refused upon his account."—(See p. 336).

[*] There is no separate map of Orion in Flamsteed's Atlas; nor is the whole of that constellation depicted in any one map. It was probably obliged to be redrawn, and reconstructed, in order to suit the subsequent arrangements.

men were renewed, and continued till the time of Mrs Flamsteed's death, which took place on July 29, 1730.

That the above mentioned circumstances attending the publication of Flamsteed's works, should never before have come to light is somewhat singular; and it is much to be regretted that some explanation was not given, at the time, of the circumstances under which they eventually appear. But that many of the facts were well known at that period, both in this country and on the Continent (although not detailed at length), appears from the testimony of contemporaneous writers; and that Mrs Flamsteed partook of the spirit and indignation of her husband, is evident from the letter which she addressed to the Vice-Chancellor of Oxford, requesting that the copy of Halley's spurious edition, presented to the Bodleian Library by Sir Robert Walpole, might be removed therefrom, as not being the genuine work of Mr Flamsteed.

On the Quadrupeds and Birds inhabiting the County of Sutherland, observed there during an Excursion in the Summer of 1834. By P. J. Selby, F. R. S. E., F. L. S. &c. &c.*

The following notices of the quadrupeds and birds inhabiting the county of Sutherland, were made during an excursion to that interesting district in the summer of 1834, expressly undertaken for the purpose of investigating its zoological productions. To enable the party† to pursue this to the greatest advantage, the sanction of the Duchess-Countess of Sutherland was requested to the undertaking, and readily obtained; and letters of introduction from Mr Losh, M. P., to the different factors upon the estate, procured the assistance of those gentlemen whose local knowledge and information was found of essential service in furthering the objects of the undertaking. From Mr Baigrie, in particular, the intelligent factor of the Scoonie and Assynt districts, much interesting information relating to the salmon-fishery was procured, as well as a detail of the experiments which for the last two years have been, and are still in active operation at the various fisheries, to determine facts of essential importance to the clearing up of the natural history of the salmon and its congeners, especially of those species that are migratory, or inhabit at times the salt as well as the fresh water; but as the ichthyology of the county has already engaged the pen of one of the party, it is unnecessary to advert to it any further at present. The accuracy of the lists, so far as they go, can be vouched for, the whole of the birds, with the exception of the *Scolopax Gallinula*, having come under the observation of the party; and the quadrupeds described were either seen alive, or their recent pelts examined, when in the possession of the fox-hunters, or regularly deputed vermin-destroyers of the districts. In a wild, mountainous, and thinly inhabited country, abounding in lochs and rivers, and whose zoology had previously been little attended to, it was naturally expected that some interesting facts connected with the breeding, distribution, &c. of various species of birds, would reward an excursion of this description. Nor were the party disappointed, for they had the pleasure of ascertaining that the various interior fresh-water lochs are selected by that beautiful bird the Black-throated Diver (*Colymbus arcticus*) for the rearing of its offspring, and both eggs and young were for the first time obtained. The Bean Goose (*Anser ferus*, Flem.) was also frequently seen with its young upon some of the larger lochs; and the Wigeon (*Mareca Penelope*), Scaup Pochard (*Fuligula marila*), and the Greenshank (*Totanus glottis*), were for the first time detected building in Britain. Among the warblers, it is interesting to trace the extensive distribution of the Willow Wren (*Sylvia trochilus*), and Sedge Warbler (*Salicaria phragmitis*), both of which are found extending their migration to the northern verge of the county, wherever situations at all suited to their habits were met with. Two or three instances of the Whitethroat (*Curruca cinerea*) were noticed at Tongue, but the want of woods, and other protecting cover, had arrested the progress of the other arboreal warblers, viz. the Blackcap (*Cur. atricapilla)*, Pettychaps *(Curruca hortensis)*, Wood Wren *(Sylvia sibilatrix)*, and Chiff-chaff *(Sylvia rufa)*; though the three first were traced as far as Dingwall, upon the northern side of the Murray Frith. Of the other forms belonging to the *Sylviadæ*, the three British species of *Saxicola* were met with, the Wheatear being remarkably abundant, and very generally spread over the county; the Redstart *(Phœnicura ruticilla)* was also twice seen. Of the *Motacillinæ*, the Pied and Grey Wagtails were generally dispersed, but the common Pipit was the only species of *Anthus* observed. The Pari or Titmice were confined to the southern confines of the county, where fir and other plantations commenced. The mountainous and rocky character of the greater part of the county, abounding as it does in cliffs of vast perpendicular height, renders it a district peculiarly favourable to the larger Raptorial birds, such as the Golden and Cinereous Eagles, Peregrine Falcon, &c.; and accordingly we find these powerful species still pretty numerous, though every device is resorted to for their destruction, on account of the havock they commit upon the flocks. The same may be said of the fox, the marten, and wild cat, which find protection in the numerous fastnesses of the rocks, and in the caves which abound in the limestone districts. The following list, copied from a document furnished by Mr Baigrie, of the foxes, martens, cats, eagles, ravens, &c. destroyed in the county within the last three years, will afford some idea of their numerous distribution, and the amount of premiums paid, the liberal inducement held out for their destruction.

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* Read before the Wernerian Natural History Society, on 21st November 1835.

† The party consisted of Sir William Jardine, Mr John Jardine, Dr Greville, Mr James Wilson, and Mr Selby. A light boat, suspended upon a four-wheeled carriage, and drawn by two horses, was the conveyance adopted, and was found particularly useful and convenient, in a country so intersected with lochs, but entirely destitute of boats. It could be shipped or unshipped at any [illegible] -fect ease, even by three of the party.

List of Vermin destroyed, and Premiums paid for the same, on the Duchess-Countess of Sutherland's estates in the County of Sutherland, from March 1831 *to March* 1834.

71 Old bitch Foxes, ...	@ 42/	=	L. 149	2	0
49 Young do. do.	@ 20/	=	49	0	0
73 Old dog do.	@ 15/	=	54	15	0
46 Young do. do. . . .	@ 7/6	=	17	5	0
901 Wild Cats, Martens, and Fumarts,	@ 2/6	=	112	12	6
418 Weasles, ...	@ 1/	=	20	18	0
Carry forward,			L. 402	12	6

		Brought forward,	L. 402	12	6
263	Otters,	@ 5/ =	65	15	0
171	Full-grown Eagles,	@ 21/ =	179	11	0
53	Young do., and Eagles eggs,	@ 10/ =	26	10	0
936	Ravens,	@ 2/ =	93	12	0
1055	Hawks,	@ 1/ =	52	15	0
1739	Carrion Crows[*] and Magpies,	@ /6 =	43	9	6
548	King-Fishers,[†]	@ /6 =	13	14	0
6323			L. 878	19	0

Under such a system, which is now supported by the lessees of the estate, it is not improbable but that, in a very few years, some of the species of the raptorial birds and carnivorous quadrupeds may become extinct. On this account, therefore, and as evidence of what existed up to a certain period, I am induced to forward these lists to the Society, being aware that in other respects they can possess but little interest to the generality of its members.

MAMMALIA.

1. Sorex araneus, *Common Shrew.* Common.——2. S. fodiens, *Water Shrew.* Common upon the margins of the lakes and rivers; all the specimens obtained were of this species.——3. Talpa Europæa, *Common Mole.* Plentiful.——4. Meles Taxus, *Common Badger.* Pretty generally dispersed throughout the midland mountainous part of the county.——5. Vulpis vulgaris, *Common Fox; large variety, with a black tip at the tail.* Foxes in Sutherland are very numerous, and of a large size, similar in form to the variety we call the Greyhound fox; but they differ from it in having the tail tipped with black instead of white. They are very destructive to the new-dropped lambs, and, when pinched for food, will even attack and kill the full grown sheep. On this account they are proscribed by the sheep farmers, and destroyed by every device by the regular appointed *fox-hunters,* who obtain the following premiums for every head they produce, viz. for a full grown dog-fox, 15s.; for a female, L. 2, 2s.; for each young fox or cub, 7s. 6d. During the day they retire to the mountains, where they lie concealed among the fragments of broken rocks, or in the holes of the debris, &c. The life of a fox-hunter, for such is the title of the vermin-destroyer, is one of great toil and fatigue, for he is obliged to be out at all seasons, and in all kinds of weather. He generally departs for the mountains under night, to be in readiness to inter-

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[*] All of the species *Corvus cornix,* Hooded Crow.

[†] The Dipper is so called throughout Sutherland and other parts of the Highlands. The *Alcedo ispida* is rarely seen.

rupt the return of his game from their predatory excursions to the lower districts. Upon such occasions he is attended by a pack of dogs, consisting of several terriers of an excellent and hardy breed, an old fox-hound or two, and a half-bred greyhound or lurcher. He is also provided with a gun, and generally carries materials to set traps, &c. for such vermin as betake themselves to holes and fissures of the rocks where they cannot be got at by his dogs. Besides the fox, he is assiduous in his pursuit of the wild cat and marten, both of which animals abound in the county, and are, like the fox, the frequent destroyers of lambs, poultry, &c. Till within the last year, the fox-hunters, or vermin-destroyers, were appointed and supported, and the premiums for the destruction of the various animals paid, by the *Duchess-Countess of Sutherland.* To each parish a distinct one was regularly appointed, and, in addition to a salary of L. 20 per annum, he was provided with a house and potato garth, and sufficient *oat-meal* for himself and ten dogs. This was exclusive of the premiums paid for the various animals destroyed, which generally amounted to as much as the stipulated money payment, and sometimes to a much larger sum, as we were informed upon good authority, that the fox-hunter of Ben Hope district had the preceding year drawn as much as L. 40 for the animals and eagles he had destroyed. By a late regulation they are now to be kept and paid by the farmers or lessees of the estate at the same rate, and with the same conditions and allowances as they enjoyed under the Noble proprietor of the county.——6. Mustela vulgaris, *Common Weasel.*——7. M. erminea, *Ermine Pole-cat.* In greater plenty than the common weasel.——8. M. putoria, *Foumart.* Common upon the banks of the rivers, margins of lakes &c.——9. M. Fagorum *aut* Abietum. Abundant in the mountainous districts, particularly where birch or other wood grows; is sometimes destructive to young lambs, and often attacks the hen-roost.——10. Lutra vulgaris, *Otter.* The otter is still pretty abundant upon the numerous lakes and rivers of this interesting county, although no opportunity is neglected of destroying them, both on account of their valuable skin and for the injury they do to the salmon fisheries. Upon the salt-water lochs or inlets they are also frequently taken, but these are said to be of smaller size, and of a lighter colour. We had not an opportunity of comparing the two together, and are therefore unable to say whether the animal that affects the salt water is merely a variety, or a species distinct from the common otter. Specimens of both kinds are however promised, and we hope ere long to be able to decide this interesting point.——11. Felis sylvestris, *Wild or Mountain Cat.* Wild cats are very numerous in this mountainous district, where they attain a great size, and sometimes commit great ravages upon the young lambs. In Assynt, upon the Ben More ridge, they are very plentiful, and find secure shelter and protection in the numerous caverns and holes of the limestone district. Of their specific distinction from the common or domestic cat no doubt is now entertained, and, in addition to the different form of the tail, other well marked characters exist. No mixed breed ever appears to take place between the species even in situations the most likely for such an event to occur.——12. Phoca Vitulina, *Common Seal.* Abounds in all the salt-water lochs, where it preys chiefly upon the Salmonidæ. The *Phoca barbata* (an inhabitant of the Fern Islands) is not known here.——13. Mus musculus, *Common House Mouse.*——14. M. sylvaticus, *Field Mouse.*——15. M. decumanus, *Brown or Norway Rat.*——16. Lepus timidus, *Common Hare.* Common in the lower lands, and the limestone district about Inch-na-Damff.——17. L. variabilis, *Alpine Hare.* Abundant throughout the central and mountainous parts of the county, and descends to the flat grounds upon the borders of Loch Shin, &c.——18. Arvicola aquatica, *Water Rat.* Common upon many of the slower running streams, and the islands of the various lochs. The black variety (at first supposed to be a distinct species) is met with about the head of Loch Naver, and a large colony of the same inhabits a low sandy island of Loch Laighal.——19. A. agrestis, *Short-tailed Field Mouse.* Common.——20. Capra Hircus, *Common Goat.* A few inhabit the fastnesses of the rocky districts, but it is doubtful if they can be regarded as indigenous.——21. Cervus elephas, *Red Deer.* This noble animal is fostered with great care, and is at present numerous in certain districts of the county. About Ben Stack, whose base is clothed with birchen woods, it may be seen in large herds, as well as about Ben Hope, Ben Laighal, &c. A hind that we suddenly came upon by the margin of Loch Shin, immediately took the water and swam across the loch, upwards of a mile in breadth. Upon Ben Stack we came upon a young fawn concealed in ferns and low underwood, crouched in its form like a hare; it bounded away with great agility at first, and was supposed to be a roe, but, after a short chase, was pulled down by a water dog that accompanied us, fortunately without receiving any very serious injury. After examination, we left it near the spot from whence it had been roused.——22. Cer. Capreolus, *Roe Deer.* This beautiful species, owing to the want of extensive woods, is not numerous in Sutherland.

(To be concluded in next Number.)

Notice concerning the Life and Writings of Professor BRINKLEY, *Bishop of Cloyne.* By M. ARAGO.

BRINKLEY having passed nearly the whole of his scientific life in Dublin, has been often taken for an Irishman. This, however, is a mistake; for he was born in England, and of English parentage. He studied at Caius College, Cambridge, where his brilliant success speedily attracted the attention of the friends of science. He was senior wrangler of his year, Malthus taking his degree at the same time. He soon became a

1814.—Appendix to the account of Observations made at the Observatory of Trinity College, Dublin, which appear to point out an annual parallax in certain fixed Stars. Read to the Royal Irish Academy March 6. 1815.—Investigations in Physical Astronomy, principally relative to the mean motion of the Lunar Perigee. Read to the Royal Irish Academy April 21. 1817.—Observations relative to the Form of the arbitrary Constant Quantities that occur in the Integration of certain Differential Equations, and also in the Integration of a certain Equation of Finite Differences. Read to the Royal Irish Academy June 23. 1817.—On the Parallax of certain Fixed Stars. Read to the Royal Society March 5. 1818.—The Results of Observations made at the Observatory of Trinity College, Dublin, for determining the Obliquity of the Ecliptic, and Maximum of the Aberration of Light. Read to the Royal Society April 1. 1819.—A method of computing Astronomical Refractions, for objects near the horizon. Read to the Royal Irish Academy, January 17. 1820.—A method for correcting the Approximate Elements of the Orbits of a Comet, and the application of the same to Observations made at the Observatory of Trinity College, Dublin, on the Comet of July 1819. Read to the Royal Irish Academy, April 17. 1820.—An account of Observations made with the eight feet Astronomical Circle, at the Observatory of Trinity College, Dublin, since the beginning of the year 1818, for investigating the effects of parallax and aberration on the Places of certain Fixed Stars. Read to the Royal Society June 21. 1821.—The Elements of Captain Hall's Comet. Read to the Royal Society January 10. 1822.—The Quantity of Solar Nutation, as affecting the N. Polar distances of the Fixed Stars, deduced from observation, and the application of this determination to confirm the conclusions relative to the parallaxes of certain Fixed Stars. Read to the Royal Irish Academy, April 1. 1822.—On the North Polar distances of the principal Fixed Stars. Read to the Royal Society December 18. 1823.—Remarks on the Parallax of *α Lyræ*. Read to the Royal Society March 11. 1824.—Results of the application of Captain Kater's Floating Collimater to the Astronomical Circle at the Observatory of Trinity College, Dublin, and remarks relative to the results. Read to the Royal Society April 27. 1826.

He also published an elemental work, Elements of Astronomy, in 1813, and 1819, which was the class-book of his course in Dublin. Each line beams with astronomy, whether it regards calculation or observation.

After having enumerated so many works on astronomy and pure mathematics, it would have been interesting to have added to this long list the titles of various papers on Botany and Legislation. I have been informed that Dr Brinkley took a deep interest in these sciences, and prosecuted them with success; but I have not had time to examine, if, in the Transactions of any of the Societies, there is any of his botanical descriptions, or discussions upon legislation.

Eruptions of the Volcano of the Cosiguina, in Nicaragua. Communicated by Colonel JUAN GALINDO.*

69

RIO MOPAN, *April* 13. 1835.

SIR,—One of the most stupendous convulsions of the globe ever known in this hemisphere took place last January, on the eruption of the volcano of Cosiguina. This volcano is situated in Nicaragua, one of the states of central America, and stands near the eastern promontory of the bay of Conchagua, separating the waters of the gulf from the Pacific. I can give no more faithful or vivid description of its appearance and effects, in the immediate vicinity, than the following translation of a report, dated January 29, from the Commandant of Union, a sea-port situated on the western shore of the bay of Conchagua, and the nearest place of any consequence to the volcano.

"On the 20th inst., day having dawned with usual serenity, at eight o'clock, towards the S. E., a dense cloud was perceived of a pyramidal figure, preceded by a rumbling noise, and it continued rising until it covered the sun, at which elevation, about ten, it separated to the north and south, accompanied by thunder and lightning. The cloud finally covered the whole firmament about eleven, and enveloped every thing in the greatest darkness, so that the nearest objects were imperceptible. The melancholy howling of beasts, the flocks of birds of all species that came to seek, as it were, an asylum amongst men, the terror which assailed the latter, the cries of women and children, and the uncertainty of the issue of so rare a phenomenon—every thing combined to overcome the stoutest soul, and fill it with apprehension; and the more so when, at four P. M., the earth began to quake, and continued in a perpetual undulation, which gradually increased. This was followed by a shower of phosphoric sand, which lasted till eight o'clock P. M. on the same day, when there began falling a heavy and fine powder like flour. The thunder and lightning continued the whole night and the following day (the 21st); and at eight minutes past three o'clock P.M. there was a long and violent earthquake, that many men, who were walking in a penitential procession, were thrown down. The darkness lasted forty-three hours, making it indispensable for every one to carry a light, and even these were not sufficient to see clearly with. On the 22d, it was somewhat less dark, although the sun was not visible; and, towards the morning of the 23d, the tremendously loud thunder claps were heard in succession, like the firing of pieces of artillery of the largest calibre, and this fresh occurrence was accompanied by increased showers of dust. From day dawn of the 23d until ten o'clock A. M., a dim light only served to shew the most melancholy spectacle. The streets, which, from the rocky nature of the soil, are full of inequalities and stones, appeared quite level, being covered with dust. Men, women, and children were so disfigured, that it was not easy to recognise any one except by the sound of their voices or other circumstances. Houses and trees, not to be distinguished through the dust which covered them, had the most horrible appearance. Yet, in spite of these appalling sights, they were preferable to the darkness into which we were again plunged from after the said hour of ten, as during the preceding days. The general distress, which had been assuaged, was renewed, and although leaving the place was attended by imminent peril from the wild beasts that had sallied from the forests and sought the towns and highroads, as happened in the neighbouring village of Conchagua and this town, into which tigers thrust themselves, yet another terror was superior, and more than half the inhabitants of Union emigrated on foot, abandoning their houses, well persuaded that they should never return to them, since they prognosticated the total destruction of the town, and fled with dismay for refuge to the mountains.

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* Silliman's Journal, July 1835.

"At half-past three on the morning of the 24th, the moon and a few stars were visible, as if through a curtain, and the day was clear, although the sun could not be seen, since the dust continued falling, having covered the ground all round about to a thickness of five inches. The 25th and 26th were like the 24th, with frequent though not violent earthquakes.

"The cause of all this has been the volcano of Cosiguina, which burst out on the 20th. I am also informed, that, on the island of Tigre, in that direction, the showers of the 21st were of pumice-stones, of the size of a pea, and some even as large as a hen's egg. The earth quaked there more than here; but no houses or other edifices have been thrown down. Here there are many people with catarrhs, headachs, sore throats, and pectoral affections, resulting doubtless from the dust. Several persons are seriously unwell, and yesterday a girl of seven years old died, with symptoms of an inflammatory sore throat. Flocks of birds are found dead, lying on the roads and floating on the sea. The showers of dust lasted till the 27th."

The following is an extract from a letter of my own, dated February 7th:—"Still in total ignorance respecting the place of the supposed volcanic eruptions of last month, I can only state my own former mistaken conjectures respecting them and others of the same class, to which they gave rise throughout Central America. Near Salama, the chief place of Verapas, being on the road from Guatemala to the port of Isabal, I distinctly heard, on the night between the 16th and 17th of January, continued noises, similar to those always produced by volcanic eruptions; however, there was something peculiar in these sounds, often resembling the discharge of single large guns.

"On the night of the 22d I was sleeping on the banks of the Polochic, about sixteen leagues before arriving at Isabal. I suppose that near eleven o'clock the apparent firing began; the guns were heard at regular intervals. Both myself and all my men had been constantly accustomed, during our whole lives, to hear volcanic eruptions in all parts of Central America, yet for some hours we were every one without a doubt that the noise was produced by artillery, and that it proceeded from the direction of Isabal. I could not but conclude that an action was taking place in that port. I then again reflecting on the improbability of such an event, raised a conjecture that the commandant, in some extraordinary state of inebriation, was celebrating his installation, his birth-day, or some other event. I slumbered and pondered on, still more completely puzzled by the long continuance of the firing. Towards day-light certainly the noise was confused, and more resembling ordinary volcanic eruptions; yet I resumed my boat journey down the river with considerable doubts on my mind, and the first canoe I met coming up the river, I ordered to be obliquely questioned as to the state of Isabal, and though the appearance of the men in it was that of fishermen, I had some ideas that they were soldiers in disguise, and that arms were concealed in the bottom of the boat. Other travellers, however, subsequently dispelled all my doubts.

"I observed nothing remarkable in the atmosphere or appearance of the night of the 22d, and no ashes such as I have since heard fell in other places; neither were ashes seen in Isabal, and the inhabitants there supposed a volcanic eruption had taken place in some mountain to their south. In Omoa they had the same idea. In Trugillo showers of ashes fell, and they supposed there that the sound proceeded from some mountains due south of them.

"In San Salvador, the federal city, the eruption was supposed to have been of the volcano of San Vicento, a day's journey to the east; the heart of the indigo country was said to be destroyed, and forty thousand inhabitants to have perished. Subsequent accounts have shewn the fallacy of all these conjectures.

"In Leon, the capital of Nicaragua, the noise of the night of the 22d was accompanied by a violent earthquake. The following day was dark, and the ashes that fell formed a layer nine inches thick. However, the loss of seven lives, and the ruin of two farms in the immediate neighbourhood of the volcano, have been the only damage done by it in that state. Persons at some distance from Quesaltenango, supposed the eruption proceeded from the volcano near that city. The noise in that direction is known to have been heard as far as Oajaca.

"At the port of Balize, in the Bay of Honduras, the British authorities there were doubtful whether the firing of the night of the 22d proceeded from a man-of-war in distress, or a naval action; in the case of the first, the superintendent ordered the guns of the fort to answer. In the interior of the settlement of Balize, the inhabitants universally believed that it was myself attacking their port with a Central American force.

"At Peten, to the westward of Balize, it was likewise supposed to be myself at the head of an independent insurrection in the British settlement.

"At Kingston, and other southern ports of Jamaica, where the sound was heard, it was supposed to proceed from the Bri- an-of-war Fly, cast on the Pedro bank. However, the ashes which subsequently fell, convinced the observers in Jamaica that a volcano was the origin.

"At Santamartha, in New Grenada, it was supposed to be the firing of the same vessel in distress. The noise was heard as far as Bogota. Captain M'Quay, who commanded the Fly, was in the harbour of Carthagena, and accompanied the governor of that part in a reconnoissance, both fearing that the firing proceeded from some vessel in want of succour. Finally, everywhere the noise was supposed to proceed from the immediate vicinity.

"In addition to the above, an official communication from the city of Nacaome, describes the pyramidal cloud on the summit of Cosiguina at half-past six A. M. It seemed of many hues and great density, and at some height separated into two parts, one spreading over the summit of Conchagua, and the other towards the peak of Perspire. Here the ground and buildings were covered to the depth of seven or eight inches with fine dust and coarse sand, in which were found birds of all kinds suffocated. Some quadrupeds from the forest sought shelter in the town, and the rivers, filled with the volcanic substances, cast upon their shores an innumerable quantity of fishes in a torpid state, and some dead.

"A letter from Omoa speaks of the earthquakes and of several eruptions, by which were wholly submerged three large towns and several petty villages, with part of the ports of St Miguel and St Salvadore. Five of the eruptions had continued for eight days, and scattered rocks, stones, and cinders in all directions, to the distance of sixty leagues. One of them burst forth within twenty miles of Truxillo, and another occurred near Balize." *

The volcanic agency seems to have operated on an extensive scale, and to have had vent in a great number of places, and the country from Bogota about 4½° N., 74° 14′ W., throughout the whole isthmus, certainly as far north as Balize (more than one thousand miles) was convulsed or affected by the concussion.

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* The terror of the inhabitants at Alencho (anticipating the approach of the last day) was so great, that 300 of those who lived in a state of concubinage were married at once.

On the Nature of the Light of Comets.

At the meeting of the Academy of Sciences, 26th October 1835, M. Arago continued his account of the physical changes observed in Halley's comet.

The weather has proved unfavourable; a clouded sky, for several successive days, having completely concealed that body from observation. It has therefore been impossible, from the observations made at Paris, to determine the question, by what kind of transformation the nebulous matter has passed from one state of agglomeration to another. To settle this point, it will be necessary to have recourse to the observations which, it is to be hoped, have been made in other places.

It was mentioned in a preceding account, that on Thursday the 15th October, a luminous sector was visible in a certain part of the head of Halley's comet; that on the day following this sector had disappeared, and that a more brilliant one, more open and of greater longitudinal extent, was formed in another place; that this second sector was observed on the 17th, when it appeared less bright, and on the 18th its weakness had decidedly increased. Since that time the comet continued concealed till the 21st. On that day, at 6¾ h. three distinct sectors were visible in the nebulosity. Of these, the weakest and least distinctly defined, was situated on the prolongation of the tail. On the 23d October all traces of these sectors had disappeared. The comet had then undergone such a change,—the nucleus, which had hitherto been brilliant and well defined, having become so large and diffuse, that the observer could scarcely believe in the reality of such a sudden and important alteration, until he had satisfied himself that the appearance was not occasioned by moisture on the glasses of his instrument. The nucleus perhaps still occupied, as on former days, the centre of the nebulosity; but the eastern region of this nebulosity was certainly much less bright than the opposite part.

From the first appearance of this comet, M. Arago had pointed out a *photometrical* method, which, if properly applied, seemed to afford certain means of determining whether these stars borrow their light from the sun, or are luminous in themselves.

This method had this peculiarity, that it was not requisite, in order to apply it, that the volume of the comet should continue constant; it was sufficient that the changes should take place with a certain degree of regularity. In this respect, however, Halley's comet proved an exception to the generality of cases; the variations in its intensity were so sudden, unexpected, and eccentric, that it would be rash to attempt to deduce any precise inferences from observations which, in ordinary circumstances, would certainly decide the point in question. On this occasion, therefore, M. Arago abandoned the idea; but, at the same time, he tried to solve the problem by means of the properties of polarised light. A few brief explanations will be sufficient to make this latter mode of investigation understood.

All direct light constantly divides itself into two pencils of the same intensity, when it traverses a crystal possessing the power of double refraction; reflected light gives, on the contrary, in certain positions of the crystal through which it is made to pass, two images of unequal intensity, provided, however, the angle of reflection is not 90°. Theoretically speaking, nothing then can appear more simple than to distinguish between direct light and that which is reflected; but in reality it is not found to be so; for in many bodies under certain angles, and in others under every possible angle, the difference in the intensity of the two images cannot be appreciated by our senses. It is necessary to add, that the rays *regularly* reflected, change their nature (became *polarised)* in the act of reflection; while those, on the contrary, which, after being absorbed, so to speak, into the substance of bodies, rendering these bodies visible in all directions, preserve with the direct light, the property of always giving two images very nearly equal. Finally, it is to be observed, that in the greater number of cases, and particularly in relation to the heavenly bodies, the regularly reflected light which reaches our eye, is such a small portion of the entire light, that it is hardly to be expected that the dissimilarity will be perceived between the two portions of the divided pencils. By using various precautions, the detail of which in this place would be without interest, M. Arago, however, always succeeded in discerning a slight difference of intensity between the two images of the brilliant comet of 1819.

It has just been stated, that the difference of the two images in the comet of 1819, was very slight; but although M. Arago's observations were confirmed by those of MM. Humboldt, Bouvard and Mathieu, it was still desirable that the result, fraught with such important consequences to astronomy, should not depend solely on a fugitive inequality of splendour. Errors of this nature, as every one is aware, are found in the works of the most celebrated natural philosophers.

M. Arago therefore modified his first apparatus in such a manner as to change the inequality of the images into a dissimilarity of colour. Thus, instead of a strong or weak image, he produced, in certain positions, a red and a green one; and in other positions, a yellow or a violet image, and all the other prismatic colours in succession. We shall not now speak of experiments, by means of which it has been ascertained that very slight differences in intensity are less easily distinguished than similar differences in colour; but every one will admit the justice of the assertion, that difference in colour is a phenomenon quite unequivocal, and leaving no doubt on the mind, while a slight inequality of splendour is very far from deserving that character.

On the 23d October, M. Arago, having applied his new apparatus to the observation of Halley's comet, immediately saw two images, presenting the complementary colours, one of them red, the other green. By turning the instrument half round, the red image become green, and *vice versa.* The light of the star, therefore, at least the whole of it, is not composed of rays possessing the property of direct light, but consists of that which is polarised or reflected specularly, that is, to speak definitively, *of light derived from the sun.*

MM. Bouvard, Mathieu, and Eugene Bouvard, an astronomical pupil in the observatory, have repeated the experiment just described, with precisely the same result. "I should be over scrupulous," adds M. Arago in the close of his verbal communication, "not to be gratified with the testimonies I have cited, and I trust that they will contribute to cause the result of my observations to be adopted; viz. that comets, as well as planets, derive their light from the sun."

Report respecting the Statistical Researches of Dr Civiale *on Calculous Affections, made to the Academie des Sciences*, by Messrs Poison, Dulong, Larrey and Double. *October 5.* 1835.

The report of these able men to the learned Academy on this important subject cannot be regarded with indifference by any of our readers, and we shall therefore now present them with such an abridgment as shall be free from professional technicalities, and will be generally interesting.

Calculous diseases, the Commissioners remark, are amongst the most intolerable that harass the human race, and more especially the life of *man*. Independent of the pains and dangers of the malady, and its cure, there are besides certain moral sensations, and grievous mental trials, which are inseparable from it, and which greatly increase the wretchedness of the sufferer. The work of M. Civiale on this subject, of which we have now to give an account, applies the method of calculation to many of the questions involved in its discussion.

As elements of his researches, M. Civiale has collected, with infinite trouble and care, a great number of tables drawn up in the midst of different populations, in large towns, and from the records of the greatest hospitals in Europe. The analysis of these tables has supplied him with the means of confirming or correcting, by the help of numerical data, many of the general pathological results deduced from the most precise clinical observation. We shall state some of these to the Academy, that it may the better appreciate the important labours of M. Civiale.

Up to the present time, it has been a prevailing opinion, that, in certain families, the parents communicate to their children a predisposition to this class of complaints; in a word, that it was *hereditary*. Now, upon this point, it is true that a very great number of facts attest that the children of such parents have, in their turn, been attacked by stone; but the facts in this list are met with yet more numerous ones in the opposite category. In the former case, too, there is this additional consideration, that the disease has attacked the children when they were placed in the same circumstances as their parents or forefathers; and this is wholly distinct from any hereditary taint. But in addition to the result from these considerations, it would be necessary, on a great scale, to ascertain the relative proportion of those who have been affected, after their progenitors, and those attacked without this condition; but sufficient facts have not yet been supplied for this proof.

It has been long known that stone in the bladder is much less frequent in women than in men. In women, too, the prospect of success from operation is much greater. The result of the numerical data on this point, go to prove that in an equal number of operations, there is a half less of men than women recover.

An extensive study of the many *causes* which are said to be capable of producing the stone, throws discredit upon numberless current assertions respecting the influence of certain viands and beverages which have been regarded as capable of inducing the malady. All still appears to be doubt and obscurity on this point.

With regard to the principal *periods of life* at which these diseases more especially assail, the numerical reports bear that more than the half occur in those who are under fourteen years of age. Pouteau states, that in the hospitals of Lyons, seven or eight children are cut for one adult. This proposition, however, is not true in all situations. Whilst it is quite true respecting Wurtemberg, the mountains of Lorraine and Barrois, for the southern aspect of the Alps, and of some counties of England, &c. it seems, on the contrary, that in other localities, and, for example, in very hot and very cold countries, adults and old men are most exposed to the affection. To this it should be added, that children thus attacked belong almost exclusively to the indigent classes of the community, whilst no such peculiarity is found with adults and the aged.

If these tables be examined regarding the *different professions* and occupations of social life, which either promote or oppose the development of these complaints, it will be found they afford no explicit instruction, nor conclusive result. They, on the other hand, shew that they are encountered nearly equally amongst duals belonging to all conditions, and that in the ratio of the relative proportions of the different professions. It is true there are many more in the indigent classes; but this is partly owing to these classes being much the most numerous, and partly because unhappily also, all the ills of life, and especially diseases, weigh most heavily upon them.

M. Civiale has arrived at nearly the same conclusions with regard to the *influence of climate.* Stone has been observed nearly equally in all countries. The contrary assertions, so often made, are the result of exaggeration and error. It is true that many circumstances, either unobserved or inaccurate, have contributed to propagate this error. If, from any circumstance, general attention was more especially directed to a particular disease, this is enough apparently to increase the number of instances. Thus, for example, during the life of the eminent lithotomist Row, it might be supposed that stone was a very common complaint in Holland, from the many operations which were then performed at Amsterdam. But after the death of Row, the number speedily diminished to one-half, and the number is now reduced to one-third.

Many other equally curious practical conclusions might be drawn from these tables. The calculous matter which collects under the influence of a catarrhus vesicæ, or other chronic affection, is usually composed of phosphate of lime, or ammoniacal magnesian phosphate. At the same time, it is not to be overlooked that calculi, in their turn, equally give rise to the above complaints. Relapses are very common, whatever method of relief has been employed. This fact was well known before the introduction of lithotritis. They occur most frequently in those who are troubled with a chronic affection. M. Civiale affirms, that all that class of diseases more speedily cease after lithotritis, than after cutting; and he adds, there is always more or less danger in repeating the operation of cutting, whilst there is scarcely any in that of pulverizing.

It is, however, especially a comparison of the different modes in which the calculi are treated and removed, that has engaged the attention of M. Civiale; and it is this principal part of his work which merits the most attention.

All the methods which have been successively employed against this cruel malady may be arranged, with sufficient accuracy, under three general heads; and each of these, though identical in their object, admit of various differences of procedure, which, however, we cannot enumerate here. *First,* It has been attempted, hitherto in vain, to dissolve the stone by the action of pretended lithontriptics, direct or indirect, general or local. *2dly,* There are the different modes of extraction by cutting; and, *3dly,* There is the bruising, *in situ,* by mechanical means. The first of these M. Civiale has passed over nearly in silence; but not so the commissioners; they wish to cherish and excite human ingenuity, and the advance of science; and remark, that between the first thought of Albucasis, to whom is probably to be referred the original idea of breaking the stone, and this important operation, now for the first time realized, and definitely inscribed in the annals of science and of art by Civiale, there elapsed first six centuries, then many a vain effort, then many a sarcasm, and much incredulity. But, notwithstanding, all this did not prevent the discovery. Who can say it will not be the same with lithontriptics?

M. Civiale institutes a comparison solely between the cutting and the breaking methods. And in the past history of the art, many a time has the power of figures been invoked to appreciate the relative value of former methods, but with little or no benefit. The 4500 operations attributed to Frère Jacques; the 1547 of Raw; the 316 of Baseillac; the 310 of Lecat; the 150 of Pouteau, which have been used as basis on which to support the surpassing excellence of the methods employed by these several practitioners, are for the most part unauthenticated facts, devoid of details, and of value. More lately analogous calculations have been published. Of these we cite those of Drs Marcet, Smith, Prout, and Yelloly, with which M. Civiale is familiar; but of these, likewise, it must be observed, that there is a want of sufficient detail and of desirable precision, and they will not even afford data by which to determine the numerical proportions of the mortality resulting from the operation.

In this work, which we are charged to make known to the academy, M. Civiale has succeeded in collecting a total of more an 5000 facts, all supplied from the practice of the principal ving surgeons of Europe. The following are the general conclusions which they supply:—Of the 5715 operations for the stone he has analyzed, he finds that 1141 have died; 4478 have been completely cured, and in about a hundred, certain unpleasant symptoms have remained. Thus, in the instance in which alone the results have been well ascertained, the mortality is about one-fifth for all ages. And here it should not be forgotten, that the half of these individuals have not attained their fourteenth year; and it is well known that at this age the probabilities of cure are at least doubled.

On the other hand, these same tables present a table of 257 individuals operated upon by lithotritis, with only six deaths, although in the whole number there were not above two or three under the age of fourteen; and this does not give one death upon every forty-two persons subjected to this operation.

And to complete the demonstration of the superiority of lithotritis over lithotomy, we shall add, that since the discovery of the former, among a very considerable number of surgeons who have been troubled with stone, scarcely one can be cited who has had recourse to lithotomy. All have been operated upon by lithotritis.

But, in sound logic, as well as in good practice, it is not on this footing that the discussion should now be placed. No one should now contend for the entire rejection of lithotomy, and the invariable substitution of lithotritis; and it should be universally allowed that in a great number of cases the latter operation would be difficult, dangerous, and impossible, and that, consequently, in these lithotomy would be preferable, and even indispensable. The question, then, accurately stated, should stand thus: What are the pathological conditions in which lithotritis promises the greatest probability of success, and what the circumstances in which it is necessary to have recourse to lithotomy? or in other terms, the formula is wanted for the respective indications of the two operations.

The commissioners remark, that they are extremely happy to have this occasion of expressing their sentiments upon the application of the doctrine of probabilities to the science of medicine. They add that it is especially questions of this kind which physicians should introduce into the *Academie des Sciences*, as there they will ever assuredly find judges both attentive and competent.

Medicine, in its extended signification, the appropriate labours of which are difficult, tedious, without eclat, and without glory, has too often sought to ally itself with the prevailing popular opinions of the day. Thus at the present time there is an attempt to apply statistics to a majority of the general questions of therapeutics. But in this case statistics are nothing else in truth but an application of the calculation of probabilities. In statistics, in other words, the different attempts to appreciate facts numerically, the first beyond all other cares should be to lose sight of man considered individually, and to regard him only as a fraction of his kind. He must be deprived of his individuality, that we may arrive at the elimination of all that this individuality may accidentally introduce into the question. In practical medicine, on the other hand, the problem is always individual, and the facts present themselves for solution only one by one. The physician has always to do solely with the personality of his patient, with a single individual with all his idiosyncrasies. For him generalities usually have nothing to do with the question.

The commissioners then point out a number of minute circumstances, necessarily connected with medicine, which renders the application of its facts to the doctrine of probability extremely difficult, if not impossible, for any practical result. They conclude:—Calculation, in truth, cannot reach the minute details of combinations which are so variable, when they are multiplied and complicated beyond a certain point. When the celebrated Morgagni, with all the power of his genius, alike able to collect facts, and to deduce from them the most judicious and just general conclusions, remarked, *Non numerandæ sed perpendendæ observationes,—facts are not to be counted but weighed,*—he energetically expressed one of the most important conditions of the doctrine of probability as applied to practical medicine.

After these reflections the reporters add, We now hasten to bestow upon M. Civiale that tribute of justice and praise which he has already often merited and obtained in the *Academie des Sciences.* We have only now to add, that this new work will furnish new proofs of the advantages presented in the majority of cases, by the substitution of an operation which is simple, easy, and free from great dangers, for another which is severe, alarming, and painful, and which, until the present time, constituted the only relief which the art afforded.

On Foot-marks of Animals in Rocks.

SUPPOSED foot-marks of animals in rocks have been mentioned by authors from an early period. Appearances of this kind have been noticed in Ayrshire and Dumfriesshire in Scotland, and in England, by Mr Allies; and Baron Humboldt and Professor Link have lately published the following observations on foot-marks of unknown animals in the variegated sandstone of Hildburghausen.

Humboldt on Foot-marks of Animals in New Red Sandstone.—After a long absence, I venture to request the attention of the Academy for a few moments to a geological fact, which is the more curious, inasmuch as it is connected with the great question of the epoch of the first appearance of the Mammiferæ upon the surface of our planet. It is now more than a year since, in a new red sandstone formation (bunte sandstein), between the village of Hisberg and the town of Hildburghausen, at the back of Thuringer Wald, the foot-marks of some great plantegrade animals were discovered, as having passed across the surface of the rock while yet in a soft state. That distinguished naturalist M. Sickler, has had the merit of first bringing these foot-marks into notice, in a letter which he addressed to M. Blumenbach. There can be no doubt that this letter is by this time well known in France; and it represents the drawings of the imprints of the feet of some quadrupeds, regarded as antediluvian. This drawing has been a second time engraved in the Archiv für Naturgeschichte of M. Wiegman (No. i. p. 127), the author of the beautiful Description of the Saurians of Mexico. The small size, and other imperfections, of M. Sickler's engraving, gave origin to many doubts concerning them; many geologists thinking that the forms of accidental concretions, such as the muschelkalk and new red sandstone often present, might have been taken for these marks in relief, moulded, so to speak, into the hollows of the imprints. These doubts, however, have disappeared from the minds of all those geologists who have seen the great stone, from ten to twelve feet long and from three to four broad, which has just been deposited in the Mineralogical Museum of Berlin, and of which I now transmit a drawing, which has been most carefully executed under the direction of M. Wiess, superintendent of the museum. That I might exhibit the phenomenon with the greater perspicuity, I have had the trace left by one animal alone drawn, out of a great many of those which had traversed the rock. Mr Weiss has distinguished among these animals, and among the smaller of them, as many as three or four different species. The route which these smaller ones followed, crosses almost at right angles, that of the great mammalia. This animal is especially remarkable for the inequality of the size of the anterior and posterior extremities. All the marks represent five toes; and the animal most probably belonged to the order Marsupialia. M. Wiegman has compared it to the Didelphis; but the conformation of the toes of the posterior extremity differ considerably from the Didelphis, the kangarroo and wombat, which have one toe almost rudimental. At Berlin we have the overlying rock, and, consequently, the foot-marks present themselves in relief. Those of the hind foot shew a foot which had been very fleshy. The animal appears to have supported the whole of its weight upon it; its step appears to have been like that of the bear; it has been a kind of amble, the small right anterior extremity is placed very regularly quite near to the right hind foot; even in the fore feet the thumb is separated from the four other toes, almost as in the quadrumana. The animal considerably resembles the form of the Phalangista, three species of which, of very large dimensions, are in the Museum of Leyden. It is for zoologists to determine whether the animal is one of the Phalangista, or if it approximates the Loris; my opinion can be of no importance. M. Sickler has found the prints of the hind feet from twelve to thirteen inches long. In another fragment of the rock in the Berlin Museum, the toes appear more slender. I have caused this mark to be drawn separately. It may perhaps be thought interesting to preserve both drawings in the Jardin des Plantes.

In the large drawing of the foot-marks of Hildburghausen, it will be observed that sinuous concretions are here and there represented, of a serpentine form. The whole of the red sandstone rock is covered with them as with a net-work; and it has been thought that these were the vestiges of the plants on which the animal trod. But the very great number of these forms throws some doubt on this: perhaps these flattened and sinuous bands are only accidental concretions, the effect of the drying and of the contraction of the softer parts of the rock. As to the prints themselves which the animal has left in its course, a single glance at the drawing—the detached toe, directed three alternate times towards the right and then towards the left, the juxtaposition of the large and small extremities, and the straight forward direction of the footmarks—appear to remove all uncertainty. Up to the present time this phenomenon, of the footmarks of an animal in a rock which was not yet hardened, has only presented itself once to the notice of geologists. I speak not of the prints of Adam's feet! nor of those of Buddha! in the island of Ceylon, nor of those of some of the early missionaries which were pointed out to my notice in the Cordilleras of the New World. I allude here, not to what concerns the fables of geology, but to facts which have been accurately observed—the foot-marks of tortoises, our acquaintance with which is due to the sagacity of Dr Buckland. (Edin Trans. vol. ii. p. 194.) What gives an especial importance to the phenomenon I have ventured to submit to the judgment of geologists is, the place which the new red sandstone formation occupies in the chronological series of secondary rocks.

Some of us may still remember the astonishment which the existence of one of the Delphides, in the Stonefield slates of the Jura or oolite formation, occasioned to the greatest and most illustrious inquirer into Nature's laws. The *keuper* and *muschelkalk* formations, and that of the new red sandstone, are placed under the oolites; and the mammalia of Hessberg, which is the subject of this note, belongs to the new red sandstone. I am aware that some geologists have been tempted to attribute these imprints to the *saurians* of the ancient world; but the plump form of the sole of its feet, and the character of the gait of crocodiles, which I have observed so often on the shores of the Orinoco, are wholly opposed to this hypothesis; and at the epoch of the monocotyledons of the coal formation, vast islands of it were dry, and might easily have afforded nourishment to the mammalia.

Professor Link has lately published a notice regarding the Hildburghausen foot-marks, but he suspects them to be impressions of *amphibious animals*, not of quadrupeds, as mentioned by Humboldt.

The plain of Hildburghausen, says Link, situated at the foot of the mountains of Thuringia, is formed of variegated sandstone, sometimes rising into little hills. This sandstone is employed for building, and it was in a quarry dug to obtain it, that a master mason named Wenzer, first noticed, about a year since, some impressions which appeared to be of an unusual description. He mentioned the circumstance to M. Sickler, who published a description of them, with figures, in a letter to M. Blumenbach. This letter appeared in the month of January in the same year, consequently a very short time after the discovery. Since that period, marks have been found in four quarries, nearly a league distant from each other, the last of them near the town of Hildburghausen. M. Weiss of Berlin and myself visited three of these quarries in the month of August of the present year, and have seen all the stones with foot-marks, which have been collected in the house of M. Wenzer and at Hildburghausen. The following is the appearance of these marks.

Immediately beneath the surface of the soil, there are alternating beds of sandstone and clay, usually about ten feet in thickness. After the removal of these beds, which do not afford stone fit for building, we come to a bed of more indurated sandstone, not exceeding half a foot in thickness, resting on a bed of clay, the thickness of which is very variable. At first nothing extraordinary is seen on this bed, unless that it presents very few fissures, and seems to be of a single piece. It is necessary to remove pieces of it, and turn them over, in order to discover the marks. They are always on the under side of the bed, and in great abundance. On causing two portions of the stone to be removed at hazard, we found very distinct marks on the under side of both. They are not impressed, but rather form knobs or nuclei, for they project from the surface in proportions varying from half an inch to three inches.

It is sometimes necessary to free the stone from the soft adhering clay, before a distinct view of the marks can be obtained. It is always the under side of the foot that is seen. The animal must therefore have made the impression in the clay, (probably when the surface was a marsh); a torrent of sand, suspended in water, had then run over it, and covering all the country, insinuated itself into the impressions. In this way, when the sand became indurated, the sandstone formed in the impressions must needs have adhered to the upper bed, and produced upon it these projecting marks. It is on this bed alone that these marks are found; they have never been observed either in the superincumbent sandstone, nor in any of the inferior beds of sandstone which have been quarried.

It is easy to distinguish the feet of four different species of animals, but I shall only speak of the most common kind, of which I have seen nearly a hundred.

Two feet are always found alike; a larger hinder one about six inches in length, and a fore one about one-half that size. They have five toes. The thumb is remote from the others, and forms nearly a right angle with them. The two thumbs of one pair of feet are always directed to the same side, but the thumbs of the following pair are directed to the opposite side; the animal must therefore have walked with an ambling gait. An extraordinary fact is, that the pairs of feet follow each other in a straight line, so that these creatures may be said to have walked *en fauchant*.

M. Weigmann, who has examined the stone bearing these marks, brought to Berlin by M. Weiss in the month of May, and who has given a notice of them in his Journal of Natural History, refers the animals which produced them to the class of Mammiferæ; but M. le Compte de Munster places them in the class Amphibia. The latter opinion appears to me to be preferable to the other. All the mammiferæ with a thumb remote from the other divisions of the foot are plantigrade; and in this instance, not the slightest vestige of a tarsus is visible, even in places where the animal appears to have slipped. The batracians have very frequently the thumb remote from the other divisions, without any conspicuous tarsus, and the anterior feet are sometimes much smaller than the hinder ones. Salamanders walk with an amble, and if none of the batracians walk *en fauchant*, that mode of progression is exemplified in cameleons, not only on trees, but likewise on land. Such are the reasons which have led me to believe, that the animals in question were either batracians or gigantic saurians.

No one who has examined these marks, particularly *in situ*, will ever conceive them to be concretions or *lusus naturæ*, which might have imposed upon naturalists. The toes often well characterized by the appearance of the joints—the anterior feet always smaller than the hinder ones—the thumb remote from the other fingers, directed sometimes to one side, sometimes to another, according to an invariable rule—and all these appearances found alike in four quarries considerably distant from each other, render it impossible that these impressions could be produced by chance.

But there are other impressions on the same stone of a more doubtful character. One frequently meets with a net of large quadrangular meshes of rounded threads, the projection of which above the surface of the stone is about half an inch. Naturalists have regarded these as crevices which have been filled with sand, in the same manner as the foot-prints. The regularity of the meshes, however, the nearly straight threads of the net, and the almost uniform thickness of these threads, do not conform to the idea of rents or crevices. They may be compared to roots, or rather rhizomes, such as those of the *Acorus calamus*, which creep on the surface of marshes, which, when decayed, would leave marks which would subsequently become filled with sand. But it has been objected that those roots do not present true anastomoses, such as are observed in this net-work. This is true; but I lately saw the roots of a *Taxus* in the botanical gallery of the Museum of Natural History, the branches of which are naturally grafted into each other, in such a manner as to form the meshes of a net. May not what has happened in this instance by chance, have once existed as a general rule in the case of certain vegetables of the primitive world?

Professor Hausmann, in a paper read before the Göttingen Society, announces the discovery of numerous foot-marks of deer, and also of a quadruped with undivided hoofs, in the travertine or old calcareous tuffa which occurs around Göttingen. In the same remarkable deposit there have been found numerous bones of deer, of small glirine animals, and also of feline species. River shells occur in the tuffa, and, what is worthy of particular notice, also fragments of ancient German clay urns, in which ashes have been found.

Memoir entitled, "Researches on the Structure and Origin of Mount Etna." By M. L. ELIE DE BEAUMONT.

THE chief object of this memoir is to make known, and to explain more exactly than has hitherto been done, the orographical "accidens" which have modified the regularity of the pyramid of Etna.

The author presented a map, four views, and a relief model of Etna, constructed in part from his own observations. It will be found, he remarks, that the map, the views, and especially the model in relief, correspond in a very small degree with the poetic image which Pindar has left us of Etna—*the pillar of heaven.* In the present memoir, the author endeavours to account for this circumstance.

The great eruptions of Etna, adds M. de Beaumont, commence by shocks of earthquakes, by which the mountain is rent in meridional planes. The walls of the vents are separated by a greater or less space,—a space which sometimes amounts to several metres. The lava, which bubbles up in the central vent, at last almost always opens for itself a passage, by which it flows out laterally on the flanks of the volcano.

When the eruption has ceased, the inferior part of each of the meridional rents remains filled with lava, which then produces a vein. As to the upper part of the rent, situated above the point of the flowing out of the lava, it often becomes filled with scoriae, or substances of "eboulement." Some of these rents, however, have remained open.

In the eruption of 1832, the phenomenon of the meridional fractures manifested itself accompanied by remarkable circumstances; and the mass of Etna became completely "etoilé." A rent cut in two the platform of the *Piano del Lago*, and changed the relative level of its two segments in such a manner as to produce at once a change of form more considerable than had resulted from the products of the eruptions of several ages, which products do not rise higher than two metres round the foundation of the *Toore del Filosofo*. This change of relative level shews that Etna does not repose on immoveable foundations, and that the segments into which it is divided by the meridional rents are susceptible of a certain alteration of position. The walls of the rents being separated, it is evident that the surface of the mountain must have been increased in dimensions, and this enlargement necessarily supposes a tumefaction. The mountain has therefore been elevated, and to an extent which might be easily calculated, if the breadth and length of the fissures were exactly known. This elevation is evidently very small, but even its existence is an important fact.

In examining the *nucleus of Etna*, the author has noticed a want of relation between the structure of the layers and their inclination; a fact diametrically opposed to that which we observe at the present day in all great currents of lava, whose form varies constantly with the amount of the inclination. According to our author, it is evident that those of the layers whose original inclination is changed, are those which at the present day are highly inclined; and that those which are nearly horizontal have, on the contrary, nearly preserved, relatively to the horizon, their original position.

The considerations, of which an analysis has just been given, prove, says the author, in terminating his memoir, that the parts of the layers of the escarpments of the *Val del Bove* which are highly inclined, are no longer in the position in which they were originally formed.

The inclination which has been acquired by some parts of this system of beds, has not been a simple movement of pressure, or the effect of dislocations purely local; but that of a tumefaction which, in elevating the whole mass of the central gibbosity, has communicated to the lateral portions an oscillatory movement.

The "soulevement" does not seem to have operated here with the same degree of simplicity as in the localities where it has given rise to *regular craters of soulevement*, such as that of the island of Palma, or the circular walls of Teneriffe and the Somma. The effort which has elevated the gibbosity of Etna seems to have acted, not at one sole and central point, but in a straight line, represented by the axis of the ellipse, of which the southern, northern, and eastern flanks of the *Val del Bove* form part; and it appears to have acted unequally on different parts of this straight line, so that its western extremity, which corresponds to the present volcanic vent, has been elevated more than all the rest. A similar soulevement could not take place without rupturing the masses so elevated, and the rents necessarily corresponded chiefly with the line of soulevement—either diverging or radiating—of its extremities; a circumstance which the memoir shews is in accordance with the phenomena as they actually occur.

The elliptical amphitheatre of the *Val del Bove* presents, then, all the characters of an *irregular crater of soulevement.* It remains to be ascertained whether this "soulevement" was gradual, or was effected suddenly and at once. The latter supposition alone seems to the author to be admissible. The nearly perfect resemblance which exists between the ejected matters composing the nucleus of the central gibbosity and those which are produced by Etna as the present day, leads to the belief that the volcanic fire acting at the present time is only the continuation of that which produced the ancient volcanic substances. For the fire not being extinct, if the "soulevement" had been gradual, there would have been a continuity and entanglement of the ancient and modern products; there would not have been that complete discordance of position between them, which constitutes one of the most remarkable features of the structure of Etna.—(Extracted from the *Compte Rendu* of the meeting of the Academy of Sciences, held on Monday, 30th November 1835.)

Phases of the Annular Eclipse of the Sun, which will happen Sunday, May 15. 1836, calculated for the Observatory of Edinburgh, Lat. 55° 57′ 20″ north, Long. 3° 10′ 54″ west.

By Mr ROBERT TREAT PAINE, of Boston, United States.

THE elements of the eclipse were computed from the lunar tables of Burckhardt, for every half hour, and from the solar tables of Carlini, corrected by Bessel, for every hour, during the continuance of the eclipse on any part of the earth. The ellipticity of the earth was considered $\frac{1}{300}$th, and the sum of the semi-diameters of the sun and moon reduced $5'''$ for irradiation and inflexion. This quantity appeared necessary from a great number of observations of the annular eclipse of February 12. 1831, and the total eclipse of November 30. 1834, made in the United States. But if this correction be rejected, the eclipse will begin earlier by $14^s.4$, and end later by $12^s.5$.

The parallaxes of the moon in latitude and longitude were computed by the method of the altitude of the nonagesimal and the apparent distance of the moon therefrom, with the greatest care; and no correction, however small, was neglected.

Mean Solar Time at Edinburgh.

	h	m	s		°	′	
Eclipse begins May 15. 1836,	1	32	54.9,	at	133	15	from vertex to the right.
Formation of the ring, . .	2	56	59.7	—	167	16	... ...
Apparent conjunction in the ecliptic, ...	2	58	59.0				
Nearest approach of centres,	2	59	5.4				
Rupture of the ring, . . .	3	1	11.5	—	63	39	from vetex to the left.
End of the eclipse, ...	4	19	8.3	—	36	12	... ...
Duration of the ring, . . .		4	11.8				
.... the eclipse, . .	2	46	13.4				

At the nearest approach of the centres of the Sun and Moon	distance of the	North limbs	73″.70
		Centres	22 .25
		South limbs	29 .20

As the semi-diameters were equally reduced for irradiation, &c., a rejection of this correction will not affect the times of the formation and rupture of the ring; but, as has been already remarked, many observations of the central eclipses of February 1831 and November 1834 shew this correction to be absolutely necessary.

ROBERT TREAT PAINE.

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr GRAHAM, Prof. of Botany.*

Acacia tristis.

A. *tristis;* stipulis setaceo-spinescentibus, deciduis; phyllodiis luridis, falcatis, nervis duobus inæqualibus, margine superiore recurvo; pedunculis subsolitariis, cumque folio longiore, ramuloque sulcato, puberulis.

Acacia tristis, *Grah.* Bot. Mag. t. 3. 420.

DESCRIPTION.—*Shrub* erect; branches drooping, puberulent, many-furrowed, when young green, afterwards brown. *Stipules* like strong rigid straight and spreading setæ, which are at first green and flattened on the sides which are towards the phyllodium, soon becoming brown, and at last falling, lateral and free at the base. *Phyllodia* very shortly petioled, suberect, dark green, slightly falcate, curving upwards except at the mucronated tip, which is more or less bent down, slightly pubescent, especially when young, undulate, having a single sessile gland on the upper edge near the base; middle rib tolerably conspicuous, branching upon its lower side; a fainter subsimple rib occurs between this and the upper edge, and rather more than half way to this last. *Capitula* solitary or very rarely in pairs, on solitary pubescent peduncles half the length of the phyllodium, and rising from the side of the bud in its axil; many-flowered, flowers yellow. *Bracteæ* greatly attenuated at the base, shortly so at the apex, marcescent. *Calyx* turbinate, 5-toothed, teeth rounded and ciliated. *Corolla* twice the length of the calyx, unequally 5-cleft, segments narrow. *Stamens* very numerous, twice as long as the corolla. *Style* lateral, longer than the stamens. *Germen* oblong, slightly compressed, yellowish-green.

This plant was raised at the Royal Botanic Garden, Edinburgh, from seeds communicated by the late Mr Fraser from New Holland, in 1828, and flowered in the greenhouse in March 1835. Its nearest affinity is to *A. undulata*, Willd., but may be easily distinguished from this by its lurid not lively green colour, by its phyllodia being longer, actually and relatively to the peduncle, by their peculiar nervation, by its more setaceous stipules, which are lateral, not inferior, and distinct, not coalescent at the base, by its capitula being generally single, very rarely in pairs, the reverse of what is observed in *A. undulata*, in which also the flowers are larger, with much more acuminated bracteæ.

It is also, and perhaps quite as nearly, allied to *A. armata*, from which it is distinguished by the smaller degree of hairiness of the branches, by the pubescent peduncles shorter than the phyllodia, and by the nervation of these. In the arrangement of the species, it ought to stand between *A. undulata* and *A. armata*.

In the present state of our knowledge, these characters must be admitted as specific distinctions, but it is not at all improbable that we shall hereafter be found to have very unduly multiplied species in this genus. In other genera, forms far more unlike than many of these are to each other, are known from their history in cultivation to be hybrids, or seedling varieties. The *Acacias* are seldom raised from the seeds of cultivated plants; and we have but an imperfect assurance that, in the wild state, they have not that mutability of form which occurs in other genera, and renders specific distinctions uncertain. These observations are particularly forced upon me by the remarkable varieties of form which exist among the different specimens of *Acacia decipiens*, *A. longifolia*, *A. stricta*, and especially *A. vernicifiua*, now in flower at the Botanic Garden, both in the greenhouse and upon the open wall.

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* The greater number of the following descriptions were put in types several months ago, but from the crowded state of the Journal, the publication has been suspended till now.

The specific name is descriptive of its drooping branches, and its dull green colour, compared with its nearest allies, but was first suggested by circumstances entirely personal, under which I write the description.

Cereus Napoleonis.

C. *Napoleonis;* ramis diffusis, repentibus, triangularibus, rarissime articulatis, repandis, tuberculis 4–5-spinosis, spinis rigidis patentibus.

Cactus Napoleonis, *Hort.*

Cereus triangularis, var. major. *Salm-Dyck.—Otto*, Allgemeine Gartenzeitung, 1833.

Description.—*Stem* much branched, branches diffused, rooting, very distantly jointed, light green, with three acute angles, and concave sides, angles tubercled, tubercles distant about 1½ inches, the intervening space being slightly repand, tubercles with 4–5 rigid stellate prickles (about 4½ lines long), having tumid bases. *Flower* (8 inches long, and, when fully expanded, 6 inches across) ascending, tube (3 inches long, 10 lines broad) green, furrowed, with rounded ridges between, having a few triangular subappressed, deep red scales, gradually enlarging upwards, and passing into the straw-coloured lanceolato-linear outer segments of the *perianth*, the inner segments of which are pure white, somewhat shorter, broader, spathulato-lanceolate, and crenate at the apex. *Stamens* numerous, yellow, declined, ascending at the apex, shorter than the perianth; anthers erect, small. *Pistil* subexserted; stigma yellow, multifid, segments subulate, spreading from their middle; style stout, cylindrical, ascending.

We received this plant from Mr Mackay at Clapton about ten years ago. It has repeatedly formed buds, but no flowers have expanded till now (September 1835). The flower expanded in the morning, and closed towards the afternoon, is very like to that of *C. grandiflorus*, and is slightly, not very agreeably, perfumed. The far greater length of its joints, their different form, and the shape of the edge between the tubercles, prevent me from considering it a variety of *C. triangularis.*

Cypella Drummondii.

C. *Drummondii;* foliis ensiformibus, plicatis; laciniis corollæ exterioribus obcordatis, interioribus naviculatis medio compressis, apice crenulatis; laciniis pistilli bifidis subulatis; caule terete, foliis longioribus.

Description.—*Tubers* fascicled, obconical, terminating in long apices, which pass into fibrous roots. *Stem* erect, round, flexuose, leafy, joints swollen. *Leaves* ensiform, plicate, distichous, shorter than the stem, sheathing at the base. *Spathe* 2-flowered, with an interposed lanceolate bractea, bivalvular; valves herbaceous, acute, unequal, the outer the smaller. *Pedicels* erect, bent at a right angle below the flower, shorter than the longer valve. *Perianth* rotate, 6-partite, purple, yellow, with brown spots in the centre, the outer segments broad, obcordate, with a small hairy point in the sinuosity, everywhere else glabrous; inner segments rather more than half the length of the outer, naviculate, crenulate at the apex, compressed laterally in the middle, and there bright yellow, with a few purple spots. *Stamens* erect, opposite to the outer segments of the perianth; filaments very short; anthers erect, lobes divaricated at both extremities, connective broad, thin, retuse, and broadest in the upper edge; pollen green, granules minute, oblong. *Pistil* longer than the stamens; stigmata 3, bifid, segments subulate, reflected; style triquetrous, enlarging towards the stigma; germen green, 3-sided, inferior.

Bulbs of this very pretty species were received at the Botanic Garden, by Dr Neill at Canonmills, and by Mr Cunningham in the nursery, Comely Bank, from Mr Drummond in 1834. They were gathered at San Filipe. The plant flowered in the stove, in all these establishments, in July 1835.

The particulars regarding the death of the indefatigable botanist and collector who greatly enriched our herbaria, and added this and many other plants to our gardens, have not yet (July 1835) reached this country, but the fact is known from letters received by Dr Hooker, and the loss will be extensively felt. Ardent, unwearied, and intelligent, with a singularly discriminating eye, and a constitution which seemed to defy climate, fatigue, and privation, no individual was ever better qualified than Drummond for the task which he enthusiastically undertook—the task of investigating and transmitting to Europe the botanical treasures of little known regions. Before he left Scotland he would willingly have braved the dangers of the Orinoco. I entreated that he would not go there, knowing certainly that his ardent mind would immediately lead him to neglect ordinary measures of precaution, and that he would quickly fall a victim to his enthusiasm. Even in a more temperate climate, he was attacked with intermittent fever; but shaking this off, and recovering from cholera, which was burying all around him, he lived till he passed to the low latitude of Cuba, from which the first account received has conveyed information of his death.

Epimedium diphyllum.

E. *diphyllum*; petiolis filiformibus, dichotomis, racemum unilateralem gerentibus, geniculis tumidis pilosis, foliolo solitario in utroque ramo; petalis planis.

Epimedium diphyllum, *Lodd.* Bot. Cabinet 1858.

Description.—*Petioles* all radical, numerous, filiform, dichotomous, sparingly covered with spreading hairs, which are more abundant at the swollen joints, each branch supporting one leafet, one of the branches occasionally trifid, and supporting three leafets. *Leafet* (length 1½ inch, breadth 9 lines) about as long as the branch of the petiole, obliquely cordate, above of lively green and glabrous, below glaucous and pubescent, about 9-nerved, reticulate, distantly provided with bristle-shaped teeth. Many of the petioles barren, others having towards the top a swollen joint, from which a single raceme springs. The portion of the petiole above this joint is equal in length to the branches of the barren petioles, and its subdivisions half of that length. *Peduncle* longer than the leaf and the portion of the petiole above its origin, without flowers for about half its length (rarely one or two in its axil) above this having about four straight, slender, glabrous, secund pedicels (about half an inch long) green and slightly swollen under the flower, the lower ones arising in the axils of small *bracteæ*, which are awanting in the upper. *Flowers* expanding irregularly along the rachis, white, cernuous, with four unequal caducous slightly coloured and dotted bracteolæ at the base. *Sepals* 4, lanceolato-oblong, spreading. *Petals* 4, obovate, rather longer than the sepals, flat. *Stamens* 4, about half as long as the sepals; anthers nearly sessile, oblong, yellow, opening by a valve rolling upwards on each side; connective green; pollen granules minute, oblong, yellow. *Pistil* green, longer than the stamens; stigma blunt, terminal; style filiform; germen oblong, gibbous on the lower side, unilocular; ovules several, obovate, attached to the dorsal suture.

The petals (nectaries, Linn.) possess a form extremely unlike that which occurs in *Epimedium alpinum*, but the variation is precisely similar to that which occurs occasionally in *Aquilegia*, and cannot form a generic distinction, where the whole habit of the plant, and the structure of every part of fructification, except the corolla, is precisely as in the common species. I have taken a different view of the petiole, and the origin of the flowers, from that which is commonly received, but it seems to me the simplest, and that which best explains the appearances. This species is a native of Japan. We received a plant at Edinburgh from Berlin in 1834. It flowered pretty freely in the greenhouse of the Botanic Garden early in spring. I do not find the hairiness of the petiole, excepting at the joints, nearly as great as is represented in the Botanical Cabinet.

Fritillaria cuprea.

F. *cuprea;* floribus solitariis, segmentis nectario ovato, internis longioribus, intus apicem versus parce pilosis; foliis ovatis, amplexicaulibus, acuminatis, erectis, sparsis, superioribus suboppositis.

DESCRIPTION.—*Tuber* ovato-orbicular, about the size of a hazel nut, covered with decayed reticulated fibres. *Stem* (15 inches high in the specimen described) erect, glabrous, round, leafy. *Leaves* (2 inches long, 4½ lines broad at the base) somewhat glaucous, ovate, acuminate, stem clasping, erect, scattered, excepting the upper pair, which are subopposite, many-nerved, the central nerve stronger, and keeled, in the axil of each leaf are two ovate tubers, diverging a little at the apices, and covered with a yellowish reticulated membrane. *Flowers* solitary, terminal, campanulate, nodding. *Perianth* of six elliptical copper-coloured segments, in two imbricated verticels, the inner the longest, and having a few harsh spreading hairs on the upper half of the inner surface towards the edges, the centre and the lower half being glabrous. The outer segments of the perianth somewhat pruinose on the outside, all the segments yellow within, and the centre of their upper half with an oblong copper-coloured mark, a little below which, on all the segments, is an ovate, nectariferous shallow pit. *Stamens* 6, about half as long as the inner segments of the perianth; filaments glabrous, subulate, slightly spreading at the apex; anthers erect, more than half as long as the filaments; pollen white, granules minute, shining, oval. *Pistil* rather shorter than the stamens, triquetrous, with three short, compressed, spreading segments at its apex, along the inside of which are the linear stigmatic surfaces.

This very graceful little plant flowered in a close greenhouse in the nursery of Mr Cunningham at Comely Bank, Edinburgh, in July 1835. He believes it was imported from Mexico.

Gentiana quinqueflora.

G. *quinqueflora;* caule ramoso, alato; floribus congestis, terminalibus; calycibus brevissimis, acutis; corolla clavata, quinquefida, laciniis aristatis, fauce nuda; foliis amplexicaulibus, deltoideo-cordatis, 3–5-nerviis.

Gentiana quinqueflora, *Pers.* Synops. Pl. 1. 285.—*Schultes,* Syst. Veget. 6 150.—*Elliott,* Bot. of S. Carolina and Georgia, 1. 341 ?—*Torrey,* Fl. of Mid. and North. Sections of United States, 288 ?—*Beck,* Bot. of North. and Middle States, 239 ?

Gentiana amarilloides, *Pursh,* Fl. Amer. Sept. 1. 186.—*Nuttall,* Genera, 1. 172.

DESCRIPTION.—*Root* annual, dichotomously branched. *Stem* (9–20 inches high) single, erect, square, winged, branched; branches decussating, spreading. *Leaves* stem-clasping, deltoideo-cordate, glabrous on both sides, palest below, 3–5-nerved, and obscurely reticulate, entire in the margin, slightly crisped, nerves prominent below. *Flowers* clustered at the extremity of the stem and branches, generally from three to five together, pedicellate, or, if single, in the axils of the leaves, it is only from the degeneration of the branches, pedicels erect. *Calyx* small, green, quinquefid, segments lanceolate, slightly spreading. *Corolla* (before expansion of the limb, 10 lines long, 3 lines in its greatest diameter, in cultivated specimens, in native specimens often smaller,) pale lilac; tube (7½ lines long) clavate; limb 5-parted, segments ovate, aristate; throat naked. *Stamens* as long as the tube, filaments adhering to the corolla as far as their middle, to which point they slightly enlarge, and then gradually contract upwards, channelled on their inner surface, unconnected with each other; anthers small, leaden-coloured, bursting on their outer surface; pollen pale, granules nearly spherical. *Pistil* as long as the stamens; stigmas small, acute; germen linear-lanceolate, greenish leaden-coloured.

This very pretty annual was raised at the Botanic Garden, from seeds sent without name by Mr Thomas Churnside, nurseryman, New York, and flowered in the greenhouse in the end of October. It was seen by Mr James Macnab growing on the grassy banks of streams among the Alleghany Mountains; and his native specimens differ in no respect from those raised at the Garden, excepting in having smaller flowers. One which I have from the collection of M. Beyrich, gathered on the Peaks of Otter, has flowers as large as the garden specimens.

From the synonyma I have excluded *Gentiana quinquefolia* of Flora Danica, because, in the plant figured there, the leaves are ovate, the flowers axillary as well as terminal, and much smaller, and because the identity of an Iceland and Virginian plant seems unlikely. I have likewise excluded the *Gentiana quinquefolia* of the various works of Linnæus; and the *Gentiana quinqueflora* of Willdenow, Lamarck, and Sprengel, because reference is by them made to Flora Danica, and because the leaves are generally described as ovate or oblong, and the stem simple. I have abstained from quoting *Gentiana amarilloides* of Michaux, because he describes his plant as smaller than *G. amarella*, with oval leaves, small lateral as well as terminal flowers of pale yellow colour, and having the segments of the limb lanceolate. In all these respects does our plant differ. I have quoted with doubt Elliott, Torrey, and Beck, on account of references they make, and some parts of their descriptions not according either with native or cultivated specimens, yet I think they must allude to the plant now described. In the other writers quoted, the references are I think sometimes mistaken, but the character is corrected.

Hakea ferruginea.

H. *ferruginea;* foliis omnibus planis obsolete crenatis, ovato oblongis, 3–5-nervibus, reticulatis, mucronatis; bracteis striatis, glabris; perianthio glabro.

Hakea ferruginea, *Sweet's* Fl. australas. t. 45.

Description.—*Shrub* erect, (specimen described 8 feet high). *Bark* brown, on the twigs covered with brown tomentum. *Branches* long, slender, drooping, somewhat flexuose. *Leaves* (2–3 inches long, 8–13 lines broad, on a free growing plant), largest below the origin of the branches, ovato-oblong, terminated with a short, stout, and sphacelated mucro, when young adpresso-pubescent, when old glabrous, 3–5-nerved, reticulate, obsoletely crenate, sessile, and with the dilated base half embracing the branches. *Fascicles* axillary, sessile. *Scales* of the flower-bud brown, membranous, nerved, concave, ciliated and diaphanous in the edge, the inner ones rhomboid and petiolate, the outer ovate and sessile. *Peduncle* and every part of the flower glabrous. *Style* erect, bearing the conical stigma (which is generally covered with the yellow granular pollen) beyond the recurved secund segments of the 4-parted perianth.

We have had this plant in the Botanic Garden under the name of *Hakea elliptica*, and we raised it from seeds sent to us from New Holland by Colonel Lindesay as *H. marginata;* it is no doubt the *H. ferruginea* of Sweet l. c., which I had overlooked, because not quoted by Brown, till pointed out to me by Dr Hooker. I can scarcely persuade myself that Brown does not notice this plant in the Supplement to the Prodr. Fl. Nov. Holland, because I understood the seeds which I received from Colonel Lindesay were collected at King George's Sound; but if he does notice it, the form has been so altered in cultivation that the character does not apply. It comes nearest Mr Brown's *H. repanda*, which I was inclined to consider it; but Dr Hooker informs me, that a specimen which he received from Cunningham, and which he believes to be *H. repanda*, is different.

Isopogon Baxteri.

I. *Baxteri*; foliis dilatato-cuneiformibus; fruticis adulti trifidis, lobis incisis, laciniis mucronatis; juvenilis indivisis, apice dentato: capitulis aggregatis: receptaculo plano.—*Br.*

Isopogon Baxteri, *Br.* Prodr. Flor. Nov. Holland. Supp. i. p. 9.

DESCRIPTION.—*Shrub* erect (specimen described 2 feet high). *Stem* round. *Bark* brown, densely covered with short soft pubescence, mixed with longer hairs, on the branches red. *Leaves* hard, stiff, with cutaneous glands on both surfaces, having pubescence and hairs similar to those on the stem, especially when young, subglabrous when old; strongly marked on both sides with elevated veins, which are generally trichotomously branched, once or twice trifid, cuneate and once or twice twisted at the base, edges placed vertically; the segments terminated with long pungent mucros; the lower leaves undivided, rounded and toothed at the apex, the teeth terminating in pungent mucres. *Capitula* crowded at the termination of the stem and branches. *Scales* of the involucre pubescent and hairy, smaller inwards, acute, reflected, subdentate. *Perianth* soft, rose-coloured, darkest at the tip, densely covered with spreading white hairs; tube very slender, segments of the limb reflexed. *Anthers* linear, yellow. *Pollen granules* subrotundo-triangular, shining, orange-yellow. *Style* as long as the perianth, fusiform at the apex, below this tumid and densely covered with yellow, reflected, crystalline pubescence. Stigmatic surface terminal.

This is a handsome species, of which seeds were sent by Colonel Lindesay, from New Holland, to the Botanic Garden, Edinburgh, in July 1830; it was raised in 1831, and flowered in the greenhouse in March and April 1835.

Liparis Walkeriæ.

L. *Walkeriæ*; foliis 2–3, subrotundo-ovatis, acutis, petiolatis, plicatis, basi obliquis, cucullatis, spica erecta multiflora brevioribus; pedunculo angulato; labello subrotundo, reflexo, crenulato; sepalis patentibus, oblongis, marginibus revolutis, germine petalisque filiformibus æquantibus.

DESCRIPTION.—Terrestrial. *Pseudo-bulbs* conical, ensheathed by about three scales (the bases of abortive leaves) dark purple. *Leaves* 2–3, with striated petioles, which are sheathing at the base, subrotundo-ovate, cucullate, acute, oblique at the base, plicate, about 11-nerved, lurid-green above, paler below. *Stalk* terminal, acutely angled. *Spike* many-flowered, cylindrical, longer than the leaves. *Rachis* green, with many waved acute angles or wings. *Bracteæ* ovato-deltoid, acute. *Germen* purple, longer than the bracteæ. *Sepals* dark purple, oblong, oblique, revolute in the edges, spreading at right angles to the germen (to which they are equal), at first nearly equidistant, afterwards, when beginning to decay, the two lower project downwards, parallel, and in contact below the lip, the third upwards, behind the column. *Petals* equal to the sepals, slender, filiform, spreading laterally, and afterwards reflexed. *Lip* subrotund, tuberculate on the upper side near the base, reflexed, dark purple in the middle, yellow and crenulate at the edges. *Column* erect, slightly curved forwards, about half as long as the sepals, purple below, colourless above, where there is a conical tooth projecting along each side of the stigma. *Anther-case* hinged at the apex, with two rounded cells, containing the sessile yellow hard pollen-masses.

We received this plant at the Royal Botanic Garden, in June 1834, from Mrs Colonel Walker, Ceylon. It has flowered twice since in the stove, and though it must certainly yield in beauty to many of the donations which I have received from the same liberal and zealous cultivator of botany, yet it is not without interest. It ought to stand in the arrange-
the species between *L. purpurascens* and *L. atropurpurea*, and is
hed from the former by its spike, and from the latter by its
gled, almost winged stem.

Primula sibirica.

P. *sibirica*, glaberrima; foliis oblongis quibusdam subrotundis, membranaceis, subrugosis, obsolete denticulatis vel integerrimis, longe petiolatis; scapo erecto, stricto, gracili; umbella pauciflora; involucro subtriphyllo, foliolis calcaratis, vaginantibus; pedunculis laxis, demum strictis et inæqualiter elongatis.

Primula sibirica, *Jacq.* Misc. Austr. i. 161.—*Willd.* Spec. Plant. i. 806.—*Roem. et Schult.* Syst. Veg. iv. 143.—*Pers.* i. 170.—*Spreng.* Syst. Veg. i. 576.—*Ledebour*, Fl. Altaic. i. 213.

Primula rotundifolia, *Pall.* It. 3.223.

Primula intermedia, *Ledebour.* Mem. de l'Acad. des Sciences de St Peters. v. 519. (var. minor.)

Primula foliis ovatis, glabris, integerrimis; umbellis paucifloris nutantibus.—*Gmel.* Fl. Sib. iv. 83. t. 46. f. 1

Description.—Whole *plant* perfectly glabrous. *Leaves* all radical, oblong, or some of the smaller ones subrotund, membranaceous, flaccid, flat, or concave at the base, of light lively green, entire in the margin or obscurely toothed, veined and slightly rugose; middle rib very strong, and forming a prominent keel behind; petioles longer than the leaves and slender. *Scape* (in our cultivated plants 8 inches to 1 foot high, in native specimens, according to Ledebour, from 3 inches elongating to nearly 1 foot), erect, straight, slender, shining. *Involucrum* generally of 3 or 4 leafets, but varying with the number of peduncles in the umbel, erect, adpressed, herbaceous, blunt or somewhat pointed, having at the base a colourless slightly spreading spur. *Peduncles* generally 3 or 4, slender, at first lax and somewhat nodding, afterwards straight, erect, parallel, and very unequally elongated (from half an inch to 2 inches). *Calyx* oblong, with five connivent short nearly blunt teeth, herbaceous, furrowed between the lobes, in appearance very nearly resembling the involucre, but herbaceous and gibbous, not toothed, at the base. *Corolla*, tube nearly twice as long as the calyx, yellowish, slightly angled, dilated at the apex; limb (8 lines across) oblique, 5-parted; segments obcordate, two-thirds of the length of the tube, reddish lilac, paler behind; throat yellow. *Stamens* sessile in the dilated apex of the tube, oblong, yellow. *Germen* ovate, glabrous, green. *Style* straight (shorter than the tube of the corolla in the specimen examined), reddish. *Stigma* globular, light green.

This species, native of marshes among the Altai mountains, about the middle of the range, was received at the Botanic Garden, Edinburgh, in 1832, from Mr Goldie of Ayr, and several specimens flowered in the cold frame and greenhouse in March and April 1835.

Pultnæa cordata.

P. *cordata;* capitulis terminalibus; foliis cordato-ovatis, acutis, mucrone pungenti, subcarnosis, concavis, utrinque glabris, stipulis scariosis.

Description.—*Shrub* erect, branches erect, red, villous, when very young green. *Leaves* crowded, petiolate, spreading, cordato-ovate, acute, terminated by a pungent bristle; veinless, somewhat fleshy, concave, glabrous on both sides, shining below, of very dark green. *Petioles* red, fleshy, adpressed, tumid at the base. *Stipules* erect, acute, adpressed, nearly twice as long as the petioles, within which they cohere, membranous. *Bracteæ* 2 at the base of the calyx, ovato-lanceolate, keeled, adpressed, free, as long as the calyx-tube. *Calyx* red, villous, teeth of the lower lip spreading and somewhat reflected, equal. *Flowers* capitate at the extremity of the branches, 2–5 in the capitula, perfumed, but not pleasantly; standard, rotundato-kidney-shaped, slightly notched, orange-coloured, with a few red streaks and spots near the claw; alæ spathulato-oblong, of the same colour and nearly as long as the standard, in contact by their upper edges, tooth short, claw linear, nearly half the length of the plate; keel red-orange-coloured at the apex, monopetalous, the linear claws only being free, apex notched, teeth short and blunt, papilla on each side distinct. *Stamens* included. *Anthers* elliptico-rotund, orange-coloured. *Pistil* equal to the longest stamens. *Germen* silky.

This plant was raised at the Botanic Garden, Edinburgh, in 1832, from seeds sent from Van Diemen's Land the year before, by Campbell Riddell, Esq. It flowered very freely in the greenhouse in April 1835, and is very ornamental, notwithstanding the lurid colour of its foliage and branches.

Sida inæqualis, *Link*.

S. *inæqualis;* fruticosa; foliis cordato-ovatis acuminatis, basi inæqualibus, crenulatis, utrinque hirtis; pedunculis petiolos longe superantibus, apice geniculatis; calycibus basi productis; corollis campanulatis; capsulis subinflatis.

Sida inæqualis, *Link. et Otto.*—Icones Plantarum selectarum Hort. Berol. p. 75. t. 34.—*Spreng.* Spec. Pl. 3. 117.

Description.—*Shrub* erect (in the specimen described slender and nearly 7 feet high). *Bark* light grey, on the young shoots green and covered with short harsh glandular pubescence. *Petioles* (1–2 inches long) alternate, spreading, round, having similar pubescence to the twigs, swollen at their apex. *Leaves* (4–7 inches long, 2–3 inches broad) slightly undulate, having on both sides a short harsh pubescence, bright green and shining above, paler and without lustre below, where in the young state it is at first white, then becoming somewhat rusty, an appearance of which scarcely a trace remains in the adult leaves, cordato-ovate, unequal at the base, acuminate, crenulate, middle rib and veins prominent on both sides, especially behind. *Peduncles* (about 2 inches long) lateral, subtended by a lanceolate, nerved, pubescent, deciduous bractea, round, geniculate near the apex, with pubescence similar to that on the petioles. *Calyx* 5-cleft; segments ovate, acute, with ferruginous pubescence, somewhat keeled and keel produced at the base. *Corolla* (1½ inch long, and when fully expanded 2 inches across) white, campanulate, petals clawed, orbiculato-ovate, delicately glanduloso-pubescent on the outside, glabrous within, shining only at the claws, many-nerved, nerves very prominent on the outside, dichotomous towards their terminations, and with smaller reticulating branches along their sides. *Stamens* and *styles* equal to the length of the petals, glabrous. *Anthers* small, yellow; pollen granules yellow, minute, globular.

We received this plant from Berlin Botanic Garden in 1829. It flowered freely for the first time in the stove of the Royal Botanic Garden, Edinburgh, in May 1835. It is said to be a native of Brazil.

Tulipa tricolor.

T. *tricolor;* bulbo solitario, caule unifloro, subdiphyllo; foliis oblongo-linearibus; petalis acutis, interioribus latioribus, basi ciliatis; filamentis supra basin barbatis, alternis longioribus pistillo parum brevioribus; capsula triquetra, mucronata.—*Ledebour.*

Tulipa tricolor, *Ledeb.* Ic. Pl. Fl. Ross. alt. illustr. t. 135.—*Ibid.* Fl. Altaica, 2. 33.

Tulipa patens, *Agardh.* in *Schult.* Syst. Veget. 7. part. 1. p. 384.

Description.—*Bulb* ovate, about the size of a filbert, covered with brown skins. *Stalk* glabrous, erect, green, (in the specimen described shorter than the leaves, in the wild specimens longer than these). *Leaves* 2 (in the plant described, and in all the specimens which I have, 5½ inches long, 3 lines broad) the upper the narrower, glabrous, glaucous and slightly channelled in front, green and somewhat keeled behind, subacute and callous at the apex. *Flower* suberect. *Petals* lanceolate, acute; outer petals narrower and rather the shorter, greenish on the outside, within white and yellow at the base, everywhere glabrous, striated; inner petals (1¼ inch long, 5 lines broad) white, yellow at the base, ciliated at the claws, everywhere else glabrous, striated with faint diverging lines, the middle rib being green. *Stamens* alternately longer, all about half as long as the petals, yellow; filament subulate, flattened, broadest above the base, and there hairy on the outside, narrower and nearly colourless below; anthers oblong, erect, nearly equal in length to the shorter filaments; pollen yellow, granules oblong. *Pistil* scarcely exceeding in length the shorter filaments, 3-sided, pyramidal; stigma of 3 obscure lobes. *Ovules* numerous, imbricated.

This species flowered in the beginning of April in the interesting collection of bulbous-rooted plants at Carlowrie, the seat of David Falconar, Esq. Ledebour notices its near relationship to *Tulipa biflora*, and I confess that, had it not been for his authority, the native specimens which I have from himself and from Dr Fischer, with the cultivated specimen before me, and the native specimens of *T. biflora* which I owe to Dr Fischer, and those cultivated in the Botanic Garden in 1828, might have left me in doubt whether they should be considered more than varieties. Among my own specimens, the distinction seems to rest chiefly on all the parts of the flower in *T. biflora* being smaller, the petals less pointed, and the outer more nearly equal to the inner in breadth, and rather longer than them. I have no means of judging as to the ripe fruit; the germen seems alike in the two. *Tulipa tricolor* is a native of dry stony places on the sides of the Altai mountains. *T. biflora* is from Astrachan.

Proceedings of the Wernerian Natural History Society.

1835, *Nov.* 21.—At this meeting (being the first of the twenty-ninth session) the following gentlemen were elected Office-bearers of the Society for the year 1836.

President.

Robert Jameson, Esq. F.R.S.L. & E., Professor of Natural History in the University of Edinburgh.

Vice-Presidents.

Sir Patrick Walker, F.L.S.
Bindon Blood, Esq. F.R.S.E.
Dr T. S. Traill, F.R.S.E.
Robert Stevenson, Esq. F.R.S.E.

Secretary.—Dr Pat. Neill, F.R.S.E. *Assist. Sec.*—T. J. Torrie, F.R.S.E.

Treasurer.—A. G. Ellis, Esq.

Librarian.—James Wilson, Esq. F.R.S.E.

Painter.—P. Syme, Esq. *Assistant.*—W. H. Townsend, Esq.

Council.

Dr John Coldstream.
David Falconar, Esq.
James Young, Esq.
William Copland, Esq. F.R.S.E.
Dr R. K. Greville, F.R.S.E.
John Sligo, Esq. F.R.S.E.
Dr Walter Adam, F.R.C.P.
Dr William Macdonald, F.R.S.E.

Sir Patrick Walker, V. P., in the Chair. Mr James Wilson read Mr P. J. Selby's Account of the Animals inhabiting the

County of Sutherland, and particularly of the Birds observed during the excursion thither of a party of naturalists in the summer of 1834.

Professor Jameson communicated a brief notice of some Observations, by M. Arago, on the Light of Halley's Comet, finally determining that cometic light is derived from the sun, and not dependent on any kind of phosphorescence inherent in the comet itself. He also made some remarks on the experiments which have been lately performed in France, on the Solidification of Carbonic Acid, and recommended the repetition of these experiments, so important in a geological view.

Dr Charles Anderson exhibited a specimen of *Cypræa guttata*, a rare species, from Java; and he also communicated a description and specimens, of a new species of Cypræa, not described by Lamarck, and which he denominated *C. castanea*. The following is the character: "Testa ovato-ventricosa, castaneo-fusca; fasciis binis, latis, obscuris, saturatioribus; marginibus incrassatis, albis, fusco-punctatis; aperturæ extremitatibus intus roseo-rubeis." Shell of a bright chestnut-brown colour on the back, the face and sides white; the latter marked with numerous spots of vivid brown of various intensity; the fauces brown, with a shade of red; length 1⅓, breadth ⅝, of an inch. Received from New South Wales, by Dr Coldstream of Leith, without any notification as to its particular locality.

There was also exhibited a male specimen of the Rocky Mountain Sheep (*Ovis montana*), which Professor Jameson had lately received from the Colombia River from Dr M. Gairdner. He remarked, that although its fur was of no value, it ought to be introduced into this country, not only from the delicacy of its flesh as food, and the fine leather to be prepared from its skin, but also from its noble figure. The Professor stated that he had many years ago brought this animal under the attention of the Society; and it was to be regretted that no steps had as yet been taken towards its introduction, it being, from its hardy nature, likely to do well in our highland mountainous districts.

Sir Patrick Walker exhibited a specimen of the moth *Phalæna* (*Geometra*) *papilionaria*, taken last summer in Aberdeenshire, and new to Scotland. He then made some remarks on its geographic distribution in England and on the continent of Europe, and mentioned several places, in the latter, where it is found in great abundance.

Dec. 5.—Dr R. K. Greville, formerly V. P., in the chair.

1. *Notice of Fossil Fishes found in the neighbourhood of Edinburgh, &c. by Professor Jameson.*—The Professor remarked that he had been induced to exhibit a part of his collection of fossil fishes to the Society, for the purpose of correcting an oversight of M. Agassiz, who states, in his work on Fossil Ichthyology, that he had received from Professor Jameson a series of fossil fishes *from Burdiehouse*, whereas none of the specimens he sent him were collected at Burdiehouse, or even in Mid-Lothian, the whole being from Fifeshire. The Professor also stated that the discovery of fossil *ichthyolites* in this neighbourhood was not of a recent date, as he had found bones and scales of fishes more than eighteen years ago in our secondary deposits, and had been in the practice for many years back, of stating the above to his pupils in the lecture-room, and pointing it out in the field. Some general observations were then made on the age of fossil fishes, their distribution in red sandstone and limestone, slate-clay, bituminous shale, and coal in the Lothians, Angusshire, Lanarkshire, &c.; and he concluded by remarking that Agassiz, after an examination of several hundred species of fishes from secondary rocks, had found *no character* whatever to distinguish fresh from salt water fishes. The species exhibited were the following: *Palaeoniscus ariolatus, ornatissimus, Robisoni*; *Eurynotus crenatus*, and *Pygopterus Jamesoni.*

Dr Traill then made some remarks on the identity of the limestone of Fifeshire with that of Burdiehouse, which he stated was proved not only from its geological position, but also from the fossil fishes which were exhibited by Professor Jameson, they belonging not only to the same genera, but all, with one exception, being of the same species as those found at Burdiehouse.

2. *On the similarity of some Birds from Northern India with European Species, by Professor Jameson.*—In continuation of his list of Birds of Northern India, nearly allied to the European, the Professor remarked, that it was his intention (already stated last year) to bring before the Society every species which should come under his observation, for the purpose of pointing out the similarity, in many respects, of the ornithology of that region with that of Europe. With this intention, therefore, he had now to lay before the Society three species, bearing a striking resemblance to the European, viz. *Saxicola rubicola*, *Sturnus vulgaris* after second moult, the bird in full plumage having been already exhibited, and

Sitta europaea; the last differing, however, in being of a deeper colour below. A fourth species was produced very nearly allied to the *Sitta Europaea*, which, however, presented characters sufficiently marked to form a new species; and from the banded tail being the most prominent, the Professor gave to it the specific name of Vitticauda. A specimen of the *Sitta frontalis* from Northern India, was also exhibited, and its wide geographic distribution pointed out, it being first found in Java, and described by Dr Horsfield.

3. It was mentioned, that the very remarkable fact of the expansion of liquefied carbonic acid, lately observed by the French academicians, has been fully verified by Mr Kemp, lecturer on chemistry, who finds that the expansion is not peculiar to this liquefied gas, but belongs to all other gases in the liquid state. At this meeting of the Society, Mr Kemp exhibited a specimen of the liquefied sulphurous acid gas, hermetically sealed in a glass tube, and separated from the materials from which it had been generated. This specimen of the liquefied gas occupied 8 inches of a tube, 5-8ths of an inch in internal diameter, and when cooled from the temperature of 60° down to 14° of Fahr., or the point at which it becomes liquid under the ordinary pressure of the atmosphere, it contracted one inch, but when heated an equal number of degrees above 60°, viz. 46°, it expanded through a greater distance than it had before contracted by the abstraction of an equal amount of caloric, shewing that the expansion went on at higher temperatures in a slightly increasing ratio, so that the expansion between its liquefying point, viz. 14° and 212°, the boiling point of water, is nearly one-third of its whole volume, the pressure against the expansion being at 212°, about 25 atmospheres. That this property does not belong to the liquefied gases exclusively, but resides equally in all other fluids, when raised above their boiling points, is shown by the following experiment; thus, ether, when raised from the temperature of 60° to 95° of Fahr., or its boiling point, undergoes an inconsiderable expansion compared with the expansion produced by an equal increase of temperature above its boiling point, when it may be said to be in the same condition with the liquefied gases in regard to pressure; and carbonic acid suffers nearly an equal expansion by an equal increasing temperature with the liquefied gases.

At this meeting Robert James Hay Cunningham, Esq. was elected a Resident Member, and Dr Martin Barry a Non-resident Member, of the Society.

The members afterwards adjourned to Dr Hope's laboratory, when Mr Kemp, Dr Hope's Experimental Assistant, exhibited an apparatus he had constructed for the repetition of the experiment on the solidification of carbonic acid; which he had, at the request of the Society, prepared for that purpose.

Proceedings of the Society for the Encouragement of the Useful Arts in Scotland.

The Society for the Encouragement of the Useful Arts commenced their sittings for the session 1835–6, in the Royal Institution, on Wednesday 18th November 1835, at 8 o'clock P. M. Mr Professor Forbes, V. P. in the Chair.

The following communications were laid before the Society:—

1. Part first, of a paper on the Construction of Oblique Arches. By Edward Sang, Esq. teacher of mathematics, &c. Edinburgh, and Councillor Soc. Arts.—A model of the centering and drawings were exhibited.

2. Specimen of Maps printed for the use of the Blind, being a map of England and Wales, by the Reverend William Taylor, York, Hon. Memb. Soc. Arts, was exhibited.

3. Drawing and Description of certain Additions to the Turning-Lathe, which facilitate the turning of Snugs, &c.: and particularly useful in slow turning, when only a small portion of the face of the body is to be turned. By Mr James Whitelaw, 18. Russell Street, Glasgow.

The following candidates were admitted as ordinary members, viz.

1. The Right Honourable Sir John Campbell, Attorney-General of England.
2. Mr Robert Grant, jun. Bookseller 82. Prince's Street, Edinburgh.

Mr Thomas Morton, Carpet Machine-maker, Kilmarnock, was admitted an Honorary Member.

December 2. 1835.—At this meeting (Colonel Macdonald of Powderhall, in the Chair), the following office-bearers were elected for the ensuing year:—

President.—His Grace the Duke of Buccleuch and Queensberry.
Vice-Presidents.—Professor Forbes, F. R. SS. L. & E., F. G. S.; Edward Sang, Esq.
Secretary.—James Tod, Esq. 21. Dublin Street.
Foreign Secretary.—Mr Alexander Adie, 58. Prince's Street.

Treasurer.—Robert Horsburgh, Esq. 15. London Street.

Curator.—Mr John Dun, 50. Hanover Street.

Ordinary Councillors.—James Jardine; George Buchanan; John Robison; Sir D. Milne; Dr D. B. Reid; Mungo Ponton; Lieut. Col. Macdonald; J. S. More; William Fraser; Wilkinson Steel; George Swinton; J. Graham Dalyell.

The following gentlemen were admitted as ordinary members :—

1. William Burn, Esq. Architect, 131. George Street, Edinburgh; 2. Mr Patrick Ritchie, Machine-maker, 56. Nicolson Street; 3. Mr J. B. Mould, Engraver, 129. High Street.

Dec. 2.—The following communications were read :—

1. Part second of a paper on the construction of Oblique Arches. By Edward Sang, Esq. teacher of Mathematics, &c. Edinburgh, and Councillor Soc. Arts.

In this part of the paper Mr Sang made some comments on the usual manner of constructing the abutments of bridges, and shewed that the effect of the ordinary form is to throw the whole pressure upon the exterior parts of the foundation. In order to equalize the pressure, he said that parabolic counter-arches ought to be introduced. In the next part of the paper, he proposed to treat of the forms of the stones, and to examine some circumstances connected with the propagation of pressures which have not hitherto been attended to.

2. Additional verbal Remarks on the Communication of Sound in Public Buildings; and on the Construction of Pulpits. By Dr D. B. Reid, lecturer on chemistry, Edinburgh, Couns. Soc. Arts.

3. Drawings of the American Patent Steam-boat, adapted to the Navigation of the Clyde. By Mr Neil Snodgrass, who fitted the machinery of the celebrated American Steam Raft Boat,—were exhibited.

List of Patents granted in Scotland from 19*th September to* 9*th December* 1835.

1835.

Sept. 19. To William Symington, of Bromley, in the county of Middlesex, cooper, for "certain improvements in paddle-wheels."

To Andrew Baldrence, chenille-cutter, residing in Paisley, for "a machine for cutting chenille cloth into chenille thread for making weft or part of weft for the shawls now called and known by the names of Chenille, Kamptschatcka, Moss, and Velours de Soie, or one or other of these names."

Oct. 16. To William Burk of Bankside, in the county of Surrey, engineer, for "certain improvements in propelling boats, ships, or other floating bodies."

To Joseph Henri Jerome Poittevin of Craven Street, in the county of Middlesex, gentleman, for an invention, communicated by a foreigner residing abroad, "of a powder which is applicable to the purpose of disinfecting night soil and certain other matters, and facilitating the production of manure."

To Patrick Seyton Hynes of Paddington, in the county of Middlesex, gentleman, for "certain improvements in wheels, axletrees, and boxes, and an apparatus for retarding or locking carriage-wheels."

22. To William Wilkinson of Lucas Street, in the parish of St George-in-the-East, and the county of Middlesex, for a "certain improvement, or certain improvements, in the mechanism or machinery by which steam power is applied to give motion to ships or other floating vessels in or through water."

23. To Charles Pierre Devaux of Fenchurch Street, in the city of London, merchant, for an invention, communicated to him by a foreigner residing abroad, for "certain improvements in smelting ironstone and iron-ore."

To William Lucy of Birmingham, in the county of Warwick, miller, for "an improvement in steam-engines."

28. To Joel Spiller of Battersea, in the county of Surrey, engineer, for "an improvement, or improvements, upon boilers for generating steam, or heating water or other fluids for useful purposes."

To Hugh Ford Bacon of Christ College, Cambridge, in the county of Cambridge, gentleman, "for an improved apparatus for regulating the flow of gas through pipes to gas-burners, with a view to uniformity of supply."

Nov. 2. To Samuel Slocum of the New Road Street, Pancras, in the county of Middlesex, engineer, for "improvements in machinery for making pins."

To Thomas Fleming Bergin of Fair View Avenue, in the county of Dublin, civil engineer, for "certain improvements in the method of suspending and adjusting the bodies of railway and all other wheeled carriages."

4. To William Longfield of Otley, in the county of York, whitesmith, for "an improved lock or fastening for doors and other situations where security is required."

To Robert Jupe of New Bond Street, in the parish of St George's Hanover Square, in the county of Middlesex, upholsterer, for "certain improvements in expanding tables, and also in ornamental, dessert, flower, and other stands."

To Elijah Galloway of Wellington Terrace, Waterloo Road, in the county of Surrey, engineer, for "certain improvements in paddle-wheels for propelling vessels."

Nov. 4. To William Patterson of Dublin, gentleman, for "an improvement in converting hides and skins into leather, by the application of matter obtained from a certain material not hitherto employed for that purpose."

6. To George Edmund Donisthorpe of Leicester, in the county of Leicester, worsted-spinner, and Henry Rawson of the same place, hosier, for "certain improvements in the combing of wool and other fibrous substances."

13. To John Birkley of High Town, near Leeds, card-maker, for "improvements in machinery for pointing wire, applicable for making of cards and pins."

17. To Richard Whiteside of Ayr, in the county of Ayr, wine-merchant, for "certain improvements in the wheels of steam-carriages, and in the machinery for propelling the same, also applicable to other purposes."

Dec. 7. To John Reynolds of Liverpool, in the county of Lancaster, gentleman, for "certain improvements in railways."

9. To Samuel Faulkner of Manchester, in the county Palatine of Lancaster, cotton-spinner, for "an improvement in the carding of cotton and other fibrous substances, by a new application of the machinery now in use for carding cotton or other fibrous substances."

To Miles Berry of No. 66. Chancery Lane, in the parish of St Andrews, Holburn, in the county of Middlesex, mechanical draftsman and patent agent, for an invention, communicated by a foreigner residing abroad, "of an improvement, or improvements, in the making or constructing of meters or apparatus for measuring gas, water, and other fluids."

SCIENTIFIC INTELLIGENCE.

1. *Sir Charles Bell.*—The Patrons of the University of Edinburgh, much to their honour, have invited Sir Charles Bell to the vacant chair of Surgery in our University; and that distinguished individual has, we understand, accepted the high distinction thus proffered to him by the citizens of his native city. Sir Charles Bell is thus, after a lapse of many years, again restored to us—is again a citizen of Edinburgh, and now a Professor in that University where he commenced those investigations which have procured for him an enduring name throughout the scientific world.

2. *Professor David Don.*—We have much satisfaction in stating, that our excellent correspondent and friend Mr David Don has been chosen Professor of Botany in King's College, London. Mr Don is son of the well known practical botanist, George Don of Forfar. He was recommended to the chair of Botany by many of the most distinguished naturalists in this country; and the chief of botanical science, Robert Brown, himself a Scotsman, warmly recommended him to the electors.

Professor Don's Flora Nepalensis is the standard work on the plants of the north of India; and his memoirs in the Transactions of the Linnean Society, and those communicated to this Journal, have been quoted by the most eminent botanists in this country and on the continent, as evincing profound acquaintance with this delightful science.

3. *Aurora Borealis.—Edinburgh, Nov.* 18. 1835; Lat. 55° 57′ N.; Long. 3° 11′ W.—Having, in common I dare say with many others, witnessed a very brilliant display of the aurora borealis last night, at 9ʰ 37ᵐ P. M. mean time, I thought of noting the direction of the luminous arch in the heavens, as well as I could, or the rapid changes which it underwent would permit. At the above time, its direction was from east towards the west, passed between Jupiter and Pollux, thence to about a degree to the south of Capella, and shot westward through Cassiopeæ, till our view of it was intercepted by the buildings of the College. Having noted the time, and its position, together with the latitude and longitude, these will furnish data, when compared with like observations elsewhere, to compute, in an approximate manner, its height and motion, and may contribute in some degree, perhaps, to illustrate the nature of these meteorological phenomena.—WILLIAM GALBRAITH.

4. *Composition of the Atmosphere.*—M. Boussingault, Chemical Professor to the Faculty of Sciences of Lyons, in the year 1834, discovered that the hydrogenous principle is mixed up with the atmosphere; but as all the experiments of this chemist were made in the city of Paris, Rue du Parc-Royal, in strict accuracy the conclusion he there drew could only be regarded as a local phenomenon. M. Boussingault has written from Lyons to M. Arago, informing him, that there also his experiments supply him with proofs of the presence of hydrogen; and that there also from one day to the next, the proportion of the gas which is contained in the air varies sometimes in the ratio of 2 to 3. M. Mattucci also has informed M. Boussingault, that, in following his process, step by step, he too has clearly determined that the air in Italy contains this principle of hydrogen. And to this statement he conjoins this important circumstance, that, in the neighbourhood of marshy grounds, the proportion of hydrogen is often three times as great as in the middle of large towns. The air of Lyons, according to very recent experiments of M. Boussingault, contains 6, 7, and even 8 parts of carbonic acid in the 10,000. This is a much larger proportion than M. de Saussure discovered in the air of the country in the neighbourhood of Geneva.

5. *Climate of Fort Vancouver.*—The winter (1833-4) has been one of some severity compared with what is usual in Britain, although not to be compared with those on the other side of the Rocky Mountains. The frost set in on the 26th December, and continued till the end of January; the lowest temperature was, + 6° Fahr. The Columbia, which is here 1660 yards wide, and 5 to 7 fathoms deep, closed when the temperature fell to 14°, and continued frozen for three weeks, permitting of free traffic

on being brought again into the cabin, only twenty-three came to life; these were at the end of four hours put once more into the air, and again hard frozen. After another week they were brought in, when only eleven were restored to life. A fourth time they were exposed to the winter temperature, and only two returned to life. On being again brought into the cabin, these two survived the winter, and in May an imperfect larva was produced from one, and six flies from the other; both of them formed cocoons, but that which produced the flies was not so perfect as the other.—*Ross's Voyage.*

10. *Polar Bear.*—During our stay at Fury Beach, many of these animals came about us, and several were killed. At that time, we were fortunately in no want of provisions, but some of our party, tempted by the fine appearance of the meat, made a hasty meal of the first one that was shot. All that partook of it soon after complained of violent headach, which, with some, continued two or three days, and was followed by the skin peeling off the face, hands, and arms; and in some, who had probably partaken more largely, of the whole body. "On a former occasion, I witnessed a somewhat similar occurrence, when, on Sir Edward Parry's Polar Journey, having lived for several days on two bears that were shot, the skin peeled off the feet, legs, and arms of many of the party. It was then attributed rather to the quantity than the quality of the meat, and to our having been for some time previous on very short allowance of provisions."—*Ross's Voyage.*

11. *The Black Whale.*—The capture of the whale, which gives employment to several thousands of our seamen, and has annually produced, on an average of the last twenty years, between eleven and twelve thousand tons of toil, and from five to six hundred tons of whalebone, has of late years greatly declined, owing to the increasing difficulties attending the fishery. Wearied by the incessant persecutions of man, the whale has lately abandoned all the accessible parts of the Spitzbergen Sea, where it was by no means unusual to see sixty or seventy sail of British vessels engaged in its capture. On the east side of Baffin's Bay, as far as the 72° of latitude, abundance of whales, of large size, were to be found some few years ago; but, like the fishery in the Spitzbergen Sea, this also was deserted. The whales retired to the westward of the then considered impenetrable barrier of ice that occupies the middle of Baffin's Bay. "In 1818, that barrier was passed by the first expedition of discovery sent by the government to those regions, where the haunts of the whale, and the nursery for its young, were laid open to the fishermen, whose daring enterprise and perseverance in following the track of the discoverers, were amply rewarded for the first few years by the most abundant success; since the produce that in any one year has been brought to England from those newly discovered portions of the Arctic Seas, is more than sufficient to cover the whole expenses of all the expeditions of discovery that have been sent during the last twenty years to these regions; and yet people

not aware of this circumstance, are perpetually asking what benefit can result to this country from such undertakings. The whale, however, still continues to retire from the persecutions of man; and the numbers of its young, which are usually destroyed without remorse by the avaricious but imprudent fishermen, must soon exhaust the fishery, and search must then be made far to the westward of Baffin's Bay, and to the eastward of Spitzbergen, for their places of retreat."—*Ross's Voyage.*

12. *Passenger Pigeon.*—A young male bird flew on board the Victory during a storm, whilst crossing Baffin's Bay in latitude 73¼° north, on the 31st of July 1829. It has never before been seen beyond the sixty-second degree of latitude; and the circumstance of our having met with it so far to the northward, is a singular and interesting fact.—*Ross's Voyage.*

13. *Spontaneous Plants.*—Few things are more extraordinary than the unusual appearance and development of certain plants in certain circumstances. Thus, after the great fire of London in 1666, the entire surface of the destroyed city was covered with such a vast profusion of a species of a cruciferous plant, the *Sisymbrium irio* of Linnæus, that it was calculated that the whole of the rest of Europe could not contain so many plants of it. It is also known that if a spring of salt water makes its appearance in a spot even a great distance from the sea, the neighbourhood is soon covered with plants peculiar to a maritime locality, which plants previous to this occurrence were entire strangers to the country. Again, when a lake happens to dry up, the surface is immediately usurped by a vegetation which is entirely peculiar, and quite different from that which flourished on its former banks. When certain marshes of Zealand were drained, the *Carex cyperoides* was observed in abundance, and it is known this is not at all a Danish plant, but peculiar to the north of Germany.—In a work upon the useful Mosses by M. de Brebisson, which has been announced for some time, this botanist states that a pond in the neighbourhood of Falain having been rendered dry during many weeks in the height of summer, the mud in drying was immediately and entirely covered to the extent of many square yards by a minute compact green turf, formed of an imperceptible moss, the *Phaseum axillare*, the stalks of which were so close to each other, that upon a square inch of this new soil, might be counted more than five thousand individuals of this minute plant, which had never previously been observed in the country.

NEW PUBLICATIONS.

1. *A Treatise on Pulmonary Consumption and Scrofulous Diseases.* By James Clark, M.D., F.R.S., &c. &c. 8vo. pp. 399. Sherwood, Gilbert, and Piper, London. 1835.

The perusal of this very interesting volume has afforded us much information, conveyed too in a most agreeable manner. We recommend it not only to the philosophical practitioner, but also to the general reader. Were medical works generally written with as much care and beauty as Dr Clark's, we would hear no more of the coarseness and pedantry of the medical philosopher.

2. *A Systematic Treatise on the Theory and Practice of Draining Land, &c. &c.* By John Johnston, Esq. Land-Surveyor. Third Edition Enlarged. One vol. 4to. pp. 225. With numerous Engravings. Edinburgh. 1835.

It is scarcely necessary on our part, to notice this now well known and very highly esteemed work, farther than to add our recommendation of it, to those which have proceeded from so many respectable quarters, and to announce the publication of a third and enlarged edition. It is a work deserving a place in the library of every landed proprietor in the country.

3. *A Manual of Select Medical Bibliography, in which the books are arranged Chronologically according to the subjects, and the derivations of the Terms, and the Nosological and Vernacular Synonyms of the Diseases, are given: With an Appendix, containing Lists of the Collected Works of Authors, Systematic Treatises on Medicine, Transactions of Societies, Journals, &c. &c.* By John Forbes, M.D., F.R.S., one of the Editors of the Encyclopedia of Practical Medicine, and of the British and Foreign Medical Review. London, Sherwood, Gilbert, and Piper. Royal octavo, pp. 403. 1835.

Dr Forbes is already so well known as an able and successful practitioner, a good naturalist, and learned physician, that it is sufficient to mention his name to secure the attention of the medical world to any work proceeding from his hands. The present volume we have found very useful, indeed is now indispensable to us, and we doubt not it has ere this time become well known to every student of medical science.

4. *A Manual of British Vertebrate Animals, or descriptions of all the animals belonging to the classes Mammalia, Aves, Reptilia, Amphibia, and Pisces, which have been hitherto observed in the British Islands; including the Domesticated, Naturalised, and Extirpated Species. The whole systematically arranged.* By the Rev. Leonard Jenyns, M.A., F.L.S., G.S., E.S., and Cambridge Philosophical Society. Cambridge, at the University Press. Longman and Co. London. 8vo. pp. 559.

This valuable work, which fully answers the high expectations formed of it, has our entire approbation. We trust it is the precursor of other treatises on the Natural History of the Animals of Great Britain.

5. *Illustrations of the Botany and other branches of the Natural History of the Himalayan Mountains, and of the Flora of Cashmere.* By J. Forbes Royle, Esq. F.R.S., M.R.A.S., &c. &c. Folio. W. and C. Allen and Co. Leadenhall Street, London.

Of this splendid and beautiful work which we have already noticed several times in this Journal, seven numbers have now appeared. It continues to increase in interest, and will, when finished, become a standard work on the subjects which it treats. Mr Royle, who, we understand, is about to return to India, will carry with him the thanks of European naturalists, for the valuable information he has communicated to them, and their best wishes for the continuance of his health in the vast field of discovery which will again be opened to him.

6. *Lethæa Geognostica; or Figures and Descriptions of the Characteristic Petrifactions of the different Rock Formations.* By Dr H. G. Bronn. 2d Part. Heidelberg, 1836.

This second Part of Dr Bronn's valuable work on Fossil Organic Remains, which has just reached us, is equally interesting with the first. We are happy to learn that its continuance is secured, as, when finished, it will form one of our best works on this important branch of geology and general natural history.

Dr Murray's Northern Flora.—An expert botanist, Dr Alexander Murray of Aberdeen, assisted by the Rev. J. Farquharson, F.R.S. of Alford, well known for his writings in the Philosophical Transactions of the Royal Society of London, and in this Journal, have announced the speedy publication of "The Northern Flora; or a Description of the Native Vegetable Productions of the North and East of Scotland, with an account of their place of growth, and their properties in Medicine and Surgery." To form two volumes in octavo.

CORRIGENDA, VOL. XIX.

Page 268, line 12 from bottom, *for* barrier *read* armour.
— 269, — 11 — *for* 2, *read* 11.
— 261, — 8 — *for* his, *read* its
— — — 13 — *for* their, *read* there.
— 377, — 6 — *for* inches, *read* perches.
— 383, — 4 — *for* parts, *read* facts.

Explanation of the Figures in Plate I, exhibiting the Minute Organization of Seven Species of the Infusoria.

Fig. 1. *Nassula elegans*, much magnified; *a*, the vent; *b*, the mouth and dental crown; *d*, the organ which is supposed to secrete the fecundating liquor; here are also to be observed many stomachic vesicles, which are rendered opaque by the violet matter secreted by the animal.

Fig. 2. The same at the moment of division, and when it has a dental apparatus in each of the two individuals.

Fig. 3. The dental crown isolated and somewhat contracted.

Fig. 4. *Nassula ornata*; *a*, the vent, whence have issued the debris of the small infusores, which have formed its aliment; *b*, the mouth and dental apparatus; *c*, the ejaculatory organ; *d*, the testicle.

Fig. 5. *Chilodon cucullulus*, (Syn. *Loxodes cucullus*), exhibiting the dental apparatus; *c*, *c*, the ejaculatory vesicles; and the stomachs filled with smaller animalcules.

Fig. 6. *Paramecium aurelia*; *b*, the mouth; *c*, *c*, ejaculatory organs.

Fig. 7. *Ophrioglena atra*, enlarged; *c*, *c*, ejaculatory organs.

Fig. 8. and 9. *Notommata clavulata*; *c*, *c*, rotatory cilia; *p*, pharynx and jaws; *e*, the stomach; *ap*. appendices of the stomach; *cl*. cloacus; *a*, vent; *ov*. ovary; *g*, *g*, nervous ganglions; *i*. the eye; *m*. the muscles; *t*. the tail.

Fig. 10. *Notommata centrura*, Eh. The same letters indicate the same parts as in the foregoing species; *b*, *b*, internal branchial organs; *b*, *c*, branchial canals.

Explanation of Plate I.

BEROE *found on the Eastern Coast of Ireland.*—Fig. 11. Front view of the lobes of the stomach, and lateral view of the tubes or sheaths of the tentacula. Fig. 12. Lateral view of the lobes of the stomach, and front view of the sheaths of the tentacula. Fig. 13. General appearance and arrangement of the cilia. They are represented somewhat shorter in proportion than they really are, in order to avoid the confused appearance inseparable from the overlapping mentioned in the paper.

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

Biographical Memoir of Dr Thomas Young. By M. Arago.*

Death which, without interruption, is thinning our ranks, seems, with a decided preference, to be directing his blows against the very limited class of our *foreign members*. Within a short space of time there has dropt from the list of the Academy the illustrious *Herschel*, whose bold ideas concerning the arrangement of the universe appear every year to be acquiring a higher probability; *Piazzi*, who, on the first day of this century, presented a new planet to our solar system; *Watt*, who was, if not the inventor of the steam-engine—for the inventor was a Frenchman—at least the discoverer of the many admirable combinations, by the aid of which the insignificant apparatus of Papin is become the most ingenious, the most useful, and the most powerful instrument of industry; *Volta*, whose electrical pile confers upon him immortality; *Davy*, alike celebrated for the decomposition of the alkalies, and for the valuable safety-lamp of miners; *Wollaston*, whom the English designate the Pope, as he never failed in his numerous experiments, or in his abstruse theoretical speculations; and, finally, *Jenner*, whose discovery I need not characterize among those who have experienced the feelings of a parent. To pay to these illustrious characters the becoming tribute of the regret, the admiration, and the gratitude of all those who have devoted themselves to science, is one of the principal duties which the Academy has imposed upon those on whom it has conferred the dangerous honour of speaking in its name on these our solemn festivals. To discharge this sacred debt with as little delay as possible, is scarcely a less imperative obligation. Gentlemen, the denizen academician always leaves after him, among the brethren to whom his election introduced him, many confidents of his private thoughts,—of the unfolding of his discoveries,—and of all the vicissitudes of his lot. The foreign member, on the contrary, is far removed from us; he but rarely takes his place amongst us, and we know little of his life, his habits, or his character, unless by the accounts of the traveller; and, when a few years have passed away, if we find any traces of these documents, we can no longer depend upon their accuracy; literary news, which are not embodied in the press, are a species of money, the circulation of which at once alters the impression, the weight, and the value.

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* Read in the French Academy of Sciences.

These reflections will at once suggest the reason why the names of Herschel, of Davy, and of Volta, have been pronounced in our meetings, before those of many celebrated academicians who have died in the midst of us; and in a few moments, I trust, there will be no one amongst us, who will not willingly concede to the universal genius, whose life I am about to recount, a full right to a similar preference.

THOMAS YOUNG was born at Milverton, in the county of Somerset, on the 13th of June 1773, of parents who belonged to the sect of Quakers. He spent his earliest years with his maternal grandfather, Mr Robert Davies of Minehead, who, though of active commercial habits, presented a rare exception, in not laying aside the cultivation of the classics. Young could read currently at the age of two years. His memory was truly extraordinary. In the time of his long sederunts at the Dame-School in the neighbourhood of Minehead, he committed to memory, at the age of four years, a great number of English authors, and even some Latin poems, which he could repeat from beginning to end, although at that time he did not understand the language. The name of Young, then, like that of many other celebrated ones already collected in our biographies, will contribute to nourish the hopes or the fears of many kind fathers, who will see, in a few lessons well recited or badly learnt, the certain tokens of a permanent mediocrity, or the infallible commencement of a glorious career. We should be losing our time if these notices of ours were to strengthen such prejudices. Accordingly, without wishing to interfere with those pure and lively emotions which are every year excited by the distribution of prizes, we may nevertheless state, that the one party may not abandon themselves to dreams which the future will never realize, and that the other may fortify themselves against discouragement, that *Pic de la Mirandole*, the phœnix of scholars in all times and countries, when in ripe years, was an insignificant author; that Newton, when keeping his terms in college, did not at all distinguish himself, and that at first, study had few attractions for him; that the first time he felt the necessity of working was to take the place of a somewhat unruly scholar, who, seated on a form superior to his own, used to kick and annoy him; that at the age of twenty-two he contended for a fellowship at Cambridge, and was beaten by a certain *Robert Uvedale*, whose name, except for this circumstance, would now have been completely forgotten; and, finally, that Fontenelle was more ingenious than accurate, when he applied to Newton these remarkable words of Lucan,—"Men have never seen the Nile, when small, and at its source."

At six years of age Young was placed under a teacher at Bristol, whose mediocrity was very fortunate for him. There is no paradox in this statement. The scholar could not accommodate himself to the slow and contracted paces of his master; he became his own instructor, and it was thus those brilliant faculties developed themselves, which, urged to any extent, would certainly have been enfeebled.

Young was eight years of age, when accident, whose influence on the events of the life of all men is more considerable than their vanity will always allow them to avow, threw him in the way of studies which were exclusively literary, and revealed to him his vocation. A respectable land-surveyor, who lived hard by, conceived a great affection for him. He sometimes, on holidays, took him along with him to the scene of his labours, and allowed him to amuse himself with his instruments. The operations, by means of which the young scholar perceived that distances of objects which were inaccessible were determined, and their elevations measured, greatly struck his imagination; but speedily a few chapters in a mathematical dictionary caused every thing which was mysterious to disappear. From this moment in his Sunday walks the quadrant took the place of the paper kite; and, in the evening, by way of relaxation, the young engineer calculated the distances which he had measured in the morning.

From the age of nine to fourteen, Young lived at Compton in Dorsetshire, with a master of the name of Thomson, whose memory was always dear to him. During these five years all his fellow-scholars were exclusively occupied, according to the custom of English schools, with a minute study of the principal Grecian and Roman authors. Young invariably maintained the first place in his class, and, at the same time, learnt the French, Italian, Hebrew, Persian, and Arabic languages. His acquisition of the two former was occasioned by his desire to satisfy the curiosity of one of his associates, who had in his possession many books which were printed in Paris, with the contents of which he was unacquainted; he learnt the Hebrew that he might read the Bible in the original; and the Persian and Arabic with the design of answering a question which had been started at the dinner-table—Whether there were as marked differences amongst the Eastern languages as there were amongst European.

I feel it necessary to state, that I am now writing from authentic documents, before I add that, whilst he was making such unheard of progress in the languages, Young, during his walks around Compton, was seized with a violent passion for botany, and that destitute of those means of magnifying, which naturalists are in the habit of using when they wish to examine the more delicate parts of plants, he undertook himself to construct a microscope, without any other help than a description of this instrument which is given by Benjamin Martin; and that to arrive at the accomplishment of his wishes, he had first to acquire much dexterity in the art of turning; and then, the algebraical formulas of the optician having presented to him symbols of which he had no idea (the symbols of fluxions), he was for a time in great perplexity; but that, finally, not wishing to give up the magnifying of the pistils and stamina, he found it easier to apprehend the differential calculus, and so to understand the unlucky formula, than to send to the neighbouring town to buy a microscope.

The eager activity of the young philosopher had seduced him beyond the limits of his strength, and, at the age of fourteen, his health was most seriously injured. The symptoms led to the belief that his lungs were affected; but the threatening appearances yielded under professional advice, and the anxious care which his relatives exercised towards the invalid.

It often happens with our neighbours beyond the Channel, that a gentleman who can afford it, when he intrusts his child to the care of a private tutor, seeks, among the boys of his acquaintance, for an associate who is already known for his abilities and acquirements. It was on this footing that Young, in 1787, became the fellow-scholar of the grandson of Mr David Barclay of Youngsbury in Hertfordshire. On the day of his arrival, Mr Barclay, who, without doubt, felt no disposition to be too exacting with a scholar of fourteen years of age, gave him a number of phrases to copy, that he might ascertain how he could write. Young, somewhat humbled perhaps by this kind of experiment, asked permission to retire into the next room. Being longer absent than the copying seemed to require, Mr Barclay began to amuse himself at the expense of the little quaker, when at length he appeared. The copy was exceedingly well written,—a writing-master could not have done it better. With regard to the delay, there was no ground for complaint, for the little quaker, as Mr Barclay called him, had not only transcribed the given English phrases, but had, moreover, translated them into *nine* different languages!

The preceptor, or, as he is called in Britain, the *tutor*, whose business it was to conduct the education of the two scholars of Youngsbury, was an eminent young man, who was then much engaged in perfecting his acquaintance with ancient languages; he was, at a future time, the author of the *Calligraphia Græca.* He was not slow to perceive the immense superiority of one of his pupils, and he recognised, with the most praiseworthy modesty, that, in their common studies, the true *tutor* was not always he who bore the name.

At this period, Young prepared, resorting always to the original sources of information, a detailed analysis of the numerous systems of philosophy which were professed in the different schools of Greece. His friends speak of this work in the highest strains of admiration; though I cannot say if the public is ever likely to receive the benefit of it. At all events, it could not be without its influence upon the life of the author, for, in giving himself up to a minute and attentive examination of the wild caprices which abounded in the conceptions of the Greek philosophers, Young felt that the partiality he had hitherto retained for the principles of the sect in which he was born gradually became more feeble, although he did not separate himself entirely from it till some years after, during his stay at Edinburgh.

The little circle of students of Youngsbury was in the habit of quitting Hertfordshire for some months during the winter, and resorting to London. On one of these occasions Young encountered an instructor who was worthy of him. He was initiated into chemistry by Dr Higgins, whose name I am the more solicitous to introduce here, because, in spite of his many inaccuracies, we should be unjust not to admit the useful part which he took in the theory of definite proportions, one of the most beautiful acquisitions of modern chemistry.

Dr Brocklesby, the maternal uncle of Young, and one of the most celebrated physicians in London, justly proud of the brilliant success of his young relative, occasionally communicated his compositions to the men of science and literature, whose approbation might have flattered his vanity. In this way Young early found himself in personal communication with the celebrated Burke and Windham, members of the House of Commons, and with the Duke of Richmond. The Duke, who was at the time Master of the Ordnance, offered him the situation of his assistant secretary. The two other politicians, though they also wished to attach him to the public service, recommended him first to go to Cambridge for a time, to study law. With so many powerful patrons, Young might readily have reckoned on one of those lucrative posts which persons in office are not slow to confer upon those who can save them from the trouble of all study or application, and who can supply them daily with the means of shining at court, in the council, or the senate, and without ever compromising their vanity by indiscretion. But Young, fortunately, had a conviction of his own powers; he felt within him the germ of those brilliant discoveries which have since so much distinguished his name; and he preferred the laborious but independent career of a man of letters, to the golden chains which were displayed before his eyes. His, then, be the honour! and let his example serve as a lesson to many of our young men whom the love of power diverts from their noble vocation, to transform them into the mere drudges of office; let them, like Young, fix their thoughts upon the future, and not sacrifice to the worthless, and withal fleeting, satisfaction of being surrounded with suitors, those proofs of esteem and gratitude which the public rarely fails to pay to intellectual labours of a high character; and if it happen that, from the illusions of inexperience, they should feel that we have prescribed for them too great a sacrifice, we would ask them to receive a lesson concerning ambition from the mouth of that great captain, whose ambition knew no bounds,—to meditate upon the words which the first consul, the hero of Marengo, addressed to one of our most honourable colleagues (M. Lemercier), at a time when he refused (a common practice with him) a very important place, that of counsellor of state:—"I understand, sir! you love letters, and you would devote yourself entirely to them. I cannot oppose this resolution. For myself, do not suppose that, had I not become generalissimo, and the instrument of the destinies of this great nation, I should ever have subjected myself to the drudgeries of office, and put myself in the state of dependence of those who do so, in the quality of minister or of ambassador? No, no! I should have devoted myself to the study of the exact sciences. I should have followed the route of Galileo and of Newton. And since I have succeeded in my grand enterprises, I should also have distinguished myself by my scientific labours. I should have left the recollection of my great discoveries. No other glory would have tempted my ambition."

Young made choice of the medical profession, in which he hoped to find fortune and independence. His medical studies were begun in London, under Baillie and Cruickshank; he continued them in Edinburgh, where Black, Munro, and Gregory then shone; and at Göttingen, in the year 1795, he took his doctor's degree. Before submitting himself to this foolish formality, which is nevertheless so imperiously required, Young, still a youth, had made himself known to the scientific world by a notice concerning the gum *Ladanum*; by the controversy he maintained with Beddoes on the subject of Crawford's theory of caloric; by a memoir concerning the habits of spiders, and the system of Fabricius, all of which were enriched by much learned research; and, finally, by a work on which I shall dwell somewhat longer, on account of its great merit, of the extraordinary favour with which it was first received, and the great neglect into which it has since fallen.

The Royal Society of London enjoys, throughout the three kingdoms, a very high and well-merited consideration. The *Philosophical Transactions* have been, for a century and a half, the glorious archives in which British genius considers it honourable to deposit its claims to the gratitude of posterity. The desire to see his name inscribed in the list of the contributors to this truly national collection, along with those of Newton, Bradley, Priestley, and Cavendish, has ever been, among the students of the celebrated universities of Cambridge and Oxford, of Edinburgh and Dublin, the most lively as the most legitimate subject of emulation. This, however, is the last limit of the ambition of the man of science; he aspires to it only on the occasion of some first-rate work; and the early essays of his youth reach the public by a way better fitted to their importance, through the means of those numerous *Journals* and *Reviews*, which have so much contributed among our neighbours to the progress of human science. Such is the common course of things; and such, consequently, was not the course of Dr Young. At the age of twenty, he sent a memoir to the Royal Society; the *Council*, composed of the most illustrious men of the day, honoured the work with its approbation, and it soon appeared in the Transactions of the Society. In it the author treated upon vision.

The subject was any thing but new. Plato and his disciples, four centuries before the Christian era, were engaged with its consideration: but at present their ideas could be cited for scarcely any other purpose than to justify the celebrated and not very flattering sentence of Cicero, "That no opinion is so absurd as not to have found some philosopher to support it."

After an interval of two thousand years, we must transport ourselves from Greece to Italy before we can find any ideas on the interesting subject of vision which deserve the attention of the historian. There (without however, like the philosopher of Ægina, having haughtily interdicted from his school those who were not geometers), the prudent experimenters pointed out the only road by which man can succeed, without committing errors, in the conquest of unknown regions; it was there that Maurolycus and Porta proclaimed to their cotemporaries, that the problem to discover *that which is*, presents so many difficulties, that it is at least presumptuous for any to throw themselves "*into the world of intelligibles*," and to seek for *what ought to be*; it was there that the two celebrated countrymen of Archimedes tried to unveil the part performed by the different *media* of which the eye is composed, and to shew that they were willing, as were Galileo and Newton afterwards, not to raise themselves above that knowledge which might be elaborated or controlled by the senses, and which, under the portico of the academy, was stigmatized by the disdainful epithet of a *simple opinion*. Such, however, is human weakness, that, after having followed, with extraordinary success, the principal inflections of the rays of light across the cornea and the crystalline lens, Maurolycus and Porta, when on the point of attaining their end, stopped all of a sudden, as before an insurmountable difficulty, when they found their theory opposed by the opinion that objects should appear to the mind reversed, if the images in the eye were turned upside down. The bold genius of Kepler, however, did not allow him to be so staggered. It was from metaphysical reasoning (psychology) that the attack originated, and it was by reasoning, clear, precise, and mathematical, that the objection was overturned. Under the powerful hand of this great man, the eye became precisely the simple optical apparatus, known under the name of the *camera obscura*; the retina the surface on which the picture is reflected; the crystalline humour being in the place of a common lens.

This comparison, which has been generally adopted since the days of Kepler, gives rise to only one difficulty. The *camera obscura*, as in a common telescope, must be put at its right focus, according to the distance of the objects. When these objects approach, it is necessary to withdraw the reflecting surface from the lens, and a contrary movement becomes necessary if the objects remove farther off. To preserve to the images, then, all the necessary distinctness, without changing the position of the surface which receives them, is impossible, unless the curve of a lens varies; that increases when we look at near objects, and diminishes when we look at distant ones.

Amongst the various methods of attaining distinct images, nature has necessarily made choice of one; for man can see with the greatest accuracy at distances which are very different. The question thus introduced has been a subject of keen research and discussion among philosophers, and many illustrious men have taken part in the debate.

Kepler, *Descartes*, and many others, have maintained that the whole of the globe of the eye is susceptible of being flattened and elongated.

Porterfield, *Zinn*, and others, have supposed that the crystalline was moveable, and, as required, could move nearer to or farther from the retina.

Jurin, *Müschenbroek*, and others, thought that there was a change in the curvature of the cornea; while *Sauvages*, *Bourdelot*, &c. also maintained a change of curvature, but only in the crystalline. This last is the opinion of Young; and two memoirs which he successively presented to the Royal Society of London, contain its complete development.

In the former, the question is investigated principally in an anatomical point of view. Here Young demonstrated, by means of direct and very delicate instruments, that the crystalline is endowed with a fibrous or muscular constitution which is admirably adapted to all varieties of change of form; and this discovery overturned the only solid objection which previously had existed against the hypothesis of Sauvages, &c. Scarcely was this discovery published, when Hunter claimed it as his own; and by so doing, the celebrated anatomist promoted the interest of the young physician, because his memoir had never been published, and had been communicated to no one. But this part of the discussion speedily lost its interest; for it was soon demonstrated that Leeuwenhoek, by means of his poweful microscopes, had previously observed and represented, in all their ramifications, the muscular fibres of the crystalline of a fish. To awaken afresh public attention, which had been exhausted by so many debates, nothing more was necessary than the high celebrity of two other members of the Royal Society, who now entered the lists. The one was a well-known anatomist, and the other the most celebrated artist of whom England can boast. They combined their efforts for the purpose of establishing unalterably the form of the crystalline lens. The learned world would not readily suspect that Sir E. Home and Ramsden together, could make inaccurate experiments, or could be deceived in microscopic measurements. Young himself thought it impossible, and therefore did not hesitate publicly to give up his theory. This willingness to allow himself beaten, so rare in a young man of twenty-five, is still rarer on the occasion of a first publication, and was therefore almost an unexampled proof of modesty. But, in truth, Young had nothing to retract. In 1800, after withdrawing his disavowal, he propounded anew his theory of the formation of the crystalline, in a paper to which, since, no serious objection has been made.

Nothing is more simple than his argument, and nothing more ingenious than his experiments. Young first rejected the hypothesis of a change in the curve of the cornea, by the assistance of microscopic observations, which would have made the slightest variations appreciable; or, to express it otherwise, he placed the eye in particular conditions, in which the change of the curve could have no effect,—he plunged it in water, and proved that then the power of seeing at different distances remained complete.

The second of these possible suppositions, that of an alteration in the dimensions of the organ, is then overturned by a series of objections and experiments, to which it would be difficult not to yield. And thus the problem becomes irrevocably settled; for who, in truth, does not see that if, of three possible solutions two have been rejected, the third becomes necessary,—that the radius of the curve of the cornea, and the longitudinal diameter of the eye, being unalterable, the shape of the crystalline itself must necessarily change? Young, however, did not stop there; he proved directly, by the minute phenomena of the disarrangement of images, that the crystalline really changes its curve. He invented, or at least improved, an instrument which might be used by persons of the least intelligence, and not at all accustomed to delicate experiments; and, armed with this new means of investigation, he convinced himself that all those who had been deprived of their crystalline lens, by its being extracted for cataract, did not possess the faculty of seeing distinctly at different distances.

We may be truly surprised that this admirable theory of vision, this net-work so cunningly wrought, where acute reasoning and most ingenious experiments so beautifully support each other, does not occupy in science the distinguished place which is due to it: but for the explanation of this anomaly there is no necessity of going very far—and Young was not, as he used often with grief to declare, another Cassander who was proclaiming new truths, which his ungrateful contemporaries refused to receive. Though less poetic, it would, I believe, be more true to remark that the discoveries of Young have never been known to the great majority of those who would have been able to appreciate them. The physiologists did not read his beautiful memoir, for it presupposes more mathematical knowledge than is usually cultivated in the faculties; and the philosophers again neglected it, in their turn, because in oral discourses or in printed works, the public scarcely requires more than those superficial views which a common mind can grasp without fatigue. In all this, whatever may have been the opinion of our learned colleague, we see nothing peculiar; like all those who sound the greatest depths of science, he has been misunderstood by the crowd; but the applause of many illustrious men should be his consolation. In a case of this sort, we should not count the suffrages, but should weigh them.

The most beautiful discovery of Dr Young, and that which will make his name imperishable, was suggested to him by an object apparently very trifling, viz. by those soap-bubbles, so brilliantly coloured, so light, and which, with difficulty escaping from the tobacco-pipe of the school-boy, become the sport of the most imperceptible currents of air. Before such an enlightened audience as this, the difficulty of producing a phenomenon, its rarity, and its usefulness in the arts, are not the necessary indications of its importance in science. I have without scruple, then, attached to the plaything of a child, the discovery which I am about to analyze, with the assurance that it will not suffer from its connection with this origin. At all events, I have no occasion to call to recollection, either the apple which, leaving the branch on which it grew, and falling unexpectedly at the feet of Newton, directed the thoughts of this great man to the simple but pregnant laws which regulate the movements of the stars; nor to the divided frog to which Physics has been recently so much indebted in the wondrous pile of Volta. In truth, without even pronouncing the name of soap-bubbles, I might have supposed that a philosopher had chosen for the subject of his experiments distilled water, that is to say, a liquid which, in its pure state, exhibits only the slightest tints of blue and green, and then only when seen in thick masses. What then, would have been the thought of his veracity, if he announced, without any further explanation, that to this limpid water he could, at will, communicate the most resplendent colours; that he knew how to make it violet, blue and green; that he knew how to make it yellow like lemon-peel, and red like scarlet, without altering its purity, or mixing it with any foreign body, or even without changing the proportions of its principal gaseous constituents? The public would assuredly consider the philosopher unworthy of all credit when, after so many strange propositions, he should moreover add, that, to produce the colour in the water, all that was necessary was to reduce it to the state of a thin pellicle; and that *thinness* may thus be regarded as synonymous with *coloured;* that the transition of each tint to a tint the most different is a necessary consequence of a simple variation of the thickness of the liquid lamina; and that this variation, in the transition of the red to the green ray, for example, is not more than the thousandth part of the thickness of a hair! Well then, these incredible theorems are nothing more than the inevitable consequences of the properties of the colouring which are presented by the soap-bubbles, and even by the very thin laminæ of all kinds of bodies.

That we may comprehend how such extraordinary phenomena have, for more than two thousand years, daily presented themselves to the eyes of philosophers, without exciting their attention, we have only to remember to how small a number of persons Nature has imparted the valuable faculty of being astonished upon fitting occasions.

Boyle was the first to open up this rich mine. He nevertheless confined himself to a minute description of the various circumstances which produce the rainbow. His associate Hooke went farther. He believed that he had discovered the cause of this kind of colouring in the mutual crossing of the rays, or, to adopt his own language, in the mutual crossings of *the waves* reflected by the two surfaces of the thin plate. This, as we shall see, was a mark of his genius; but it could not be appreciated at a time when the complex nature of white light was not yet known.

Newton made the colours of thin plates the object of his favourite study. He devoted a whole book of his celebrated treatise on optics to them; and established the laws of their formation by an admirable suite of experiments which has hitherto been excelled by no one. By illuminating with homogeneous light the regular small coloured rings which Hooke had already mentioned, and which were produced around the point of contact of two superimposed glass-lenses, he proved that, for each kind of simple colour, there exists, for thin laminæ of every sort, certain thicknesses at which no light is reflected. This result was of fundamental importance, as it contained the key of all these phenomena.

Newton was less happy in the theoretic views which this remarkable observation suggested to him. To say, with him, of a luminous ray which is reflected, that it is *in a fit* of easy reflection; and to say of a ray which entirely traverses the plate that it is *in a fit* of easy transmission, is nothing more than to announce in obscure terms that which the experiment of the two lenses had previously shewn.

The theory of Thomas Young was not liable to this objection. In it he did not admit *fits* of any kind, as an original property of rays; and the thin plate was moreover assimilated, in all respects, to a thick mirror composed of the same materials. If in some of these points, no light was seen, Young did not thence conclude that its reflection had ceased in these points, but supposed that, in the peculiar directions of these points, the rays reflected by the second surface, encountering the rays reflected by the first, *were completely annihilated.* It is this conflict of the rays which the author designated by the name, now so famous, of *interference.*

This, then, beyond contradiction is a most strange hypothesis. Who would not be surprised to find darkness in the sun's rays, —in points which the rays of the luminary freely reach; and who would imagine that any one could suppose that the darkness could be produced by light being added to light!

A philosopher may well be proud when he can announce a result, which to such an extent as this surprises our common apprehensions; but he ought, without delay, to support it with demonstrative proofs, under the risk of being compared to those Eastern writers, whose wild reveries so much delighted for a thousand and one nights the sultan *Schahriar.*

But Young had not this prudence; and at first he only demonstrated that his theory would explain the phenomena, and proceeded no farther than possibilities. So that when, at a later period, he proceeded to his positive proofs, the public had adopted prejudices which he could no longer overcome. It is nevertheless true that the experiment upon which our learned colleague at that time founded his memorable discovery, places it beyond the shadow of a doubt.

Two rays coming from the same source may proceed by routes somewhat different, and may cross at a certain point in space. If in this point we place a sheet of fine paper, each ray, taken alone, will illuminate it brightly; but when the two rays unite—when they arrive simultaneously upon the paper—all illumination disappears, and the most complete darkness takes the place of light.

Two rays do not, however, always completely destroy each other in the point of intersection. Sometimes there is only a partial obscuration; and sometimes the one ray is added to the other. Every thing depends on the difference of the lengths of the course which they have run; and that according to very simple laws, the discovery of which would at any time have been sufficient to have immortalized any philosopher.

The differences in the course which produces among rays those conflicts which are accompanied with their entire destruction, differ in amount for lights differently coloured. When two white rays cross each other, it is then possible that one of their principal component parts, the red ray for example, may alone be in those circumstances in which it will be destroyed. But white deprived of red becomes green! This luminous interference, then, manifests itself by the phenomena of coloration; and thus the different elementary colours exhibit themselves, without a prism having separated them. And now then, let it be well considered, that there does not exist a single point of space where a thousand rays of the same origin do not cross after being reflected more or less obliquely; and then we may at once perceive all the extent of that unexplained region which the doctrine of interferences opens up to the investigations of philosophers.

At the time when Dr Young published this theory, many of the phenomena of recurrent colours had already presented themselves to the attention of observers; and, we must add, had remained wholly unexplained. Among the number we may mention those which are formed by reflection, not more on thin pellicles than on thick mirrors of glass which are somewhat curved; also the iridescent bands of different sizes with which the shadows of bodies are fringed externally, and sometimes covered internally, which Grimaldi first perceived, and which also exercised the genius of Newton, but the satisfactory theory of which was proposed by Fresnel; also the red and green coloured arches, more or less numerous, which are seen immediately underneath the seven prismatic colours composing the *principal* rainbow in the sky, and which had appeared so completely inexplicable, that philosophers had ceased to make any mention of them in the common treatises on physics; and finally, those coronas of marked colours, with diameters perpetually varying, which often appear round the sun and the moon. And when I remember that there are those who do not value scientific theories, except so far as they have an immediate practical application, I must not terminate this enumeration of the phenomena which characterize the more or less numerous series of periodic colours, without mentioning those rings which are so remarkable for the regularity of their form, and the purity of their colours, with which every strong light seems to be surrounded when it is looked at through a mass of floating motes, or of minute filaments which are of equal dimensions. It was these rings in truth, that suggested to Young the idea of an extremely simple instrument which he called *Eriometer*, and with which we can measure without difficulty the dimensions of the most minute bodies. The Eriometer, which is yet but little known among observers, has an immense advantage over the microscope, in that, in a moment, it supplies us with the *mean size* of the millions of particles which are found in the field of vision. It, moreover, possesses the singular property of giving no indication when the particles differ very much among themselves, and when the determination of the question could be of no real benefit. Young applied his eriometer to the measurement of the globules of the blood in the different classes of animals,—to that of the fine dust of various species of vegetables, and also to measure the fineness of the fleecy fibres employed in the manufacture of goods, from that of the hair of the beaver, the finest of all, to that of the fleece of the sheep of the Sussex breed, which was placed at the other extremity of the scale, and the filaments of which were four and a half times as thick as the hair of the beaver.

Previous to the time of Young, the numerous phenomena respecting coloration, to which we have been alluding, were not only inexplicable, but could not in any way be associated with each other. Newton, for example, who was so long engaged with the subject, did not see any connection between the irises of these laminæ, and the colours of the spectrum. Young shewed that these two kinds of coloured striæ were only different effects of interference. At a subsequent period, when chromatic polarization had been discovered, he deduced from some measurements of thickness, some remarkable numerical analogies, which led to a strong presumption that, sooner or later, this extraordinary kind of polarization will be connected with his doctrine. Before that takes place, however, we must allow, there is a great void to fill up. Important properties of light which were then wholly unknown, did not allow the supposition that in certain crystals, &c. double refraction would produce peculiarities by those destructions of light which result from the crossings of two pencils; Young, however, had the honour of opening the way in this career, and he commenced the deciphering these hieroglyphics of optics.

(To this succeeds an account of his discoveries in hieroglyphics, which we presented to our readers in our 19th volume, and on which now, therefore, we need not dwell.)

The limits to which I am confined, do not allow me even to quote the simple titles of many of the writings which Dr Young published. The mere announcement of so rich and varied a catalogue, is sufficient to establish the high character of our learned associate. Who is there, in truth, but would suppose, that we were giving the list of the labours of several academies, and not those of a single person, when he heard the following enumeration of distinct titles;—Memoir on the forges, where iron is wrought—Essays on music and painting—Researches on the habits of spiders, and on the system of Fabricius—On the stability of the arches of bridges—Upon the atmosphere of the moon—Description of a new species of opercularia—Mathematical theory of epicycloidal curves—On the restoration and translation of certain Grecian inscriptions—On the means of strengthening the frame-work of ships of the line—On the play of the heart and arteries in the phenomena of circulation—Theory of tides —On the diseases of the chest—On friction in the axis of machines—On the yellow fever—On the calculation of eclipses—Essay on grammar, &c.

These labours, so numerous and varied, would seem to have required the laborious and retired life of one of those geniuses who is almost lost to those around him, and who, from their early youth, detach themselves from all their cotemporaries, and bury themselves in their studies. But far from this, Young was what is usually called a man of the world. He was a frequent ornament of the most brilliant circles in London. The graceful elegance of his mind and manners, would have amply sufficed to have conferred distinction in these; but when we moreover consider that in these numerous meetings there may be some fifty subjects which, in their turn, are all discussed in a few hours, the value of such a living library will be easily perceived, where every one could at the moment find an accurate and satisfactory answer upon questions of every kind which might be proposed to him.

Young was deeply conversant with the arts. Many of his memoirs testify the profound knowledge he had early acquired of the theory of music. He was also an excellent performer, and I believe there was not a couple of known instruments, even including the Scotch bagpipe, on which he could not play. His taste for painting appeared during his stay in Germany. It was there that the magnificent collection of Dresden wholly absorbed him, for he did not only aspire to the easy acquirements of knowing to a certainty the name of the painter of every picture, but the characteristic features and defects of all the great masters, their frequent changes of style, the material objects with which they worked, and all the modifications which these objects, and amongst others the colours, underwent in the course of time, occupied him in their turn. In a word, Young studied painting in Saxony, as formerly he had studied languages in his own country; and as afterwards he studied all the sciences. Every thing was in his view a subject of meditation and research. His college companions used to narrate a laughable example of this tendency of his mind. They state that one day on going into his room at Edinburgh, after he had for the first time received his dancing master's instructions on the minuet, they found him engaged in tracing, with rule and compass, the various crossings of the two dancers, and the various improvements which he thought might be introduced.

Young very early borrowed from the Quakers, to whose sect he then belonged, the opinion that the intellectual faculties of children usually differ far less than is usually supposed. *That which one man has done another may do*, had become his favourite maxim, and never did he decline any personal proofs to which he was desired to submit his system. The first time he mounted on horseback, in company with the grandson of Mr Barclay, the groom who followed them cleared a six-bar gate. Young immediately tried to follow him, but was thrown to the distance of ten feet. He got up without saying a word, made a second attempt, was again unseated, but stuck to his steed; a third time he tried it, and as his favourite thesis required, succeeded in doing what had been done before him. This experiment need not have been cited here, had it not been again repeated first at Edinburgh, and then at Göttingen, and pushed farther than would perhaps be generally credited. In one of these towns, Young, after a very short preparation, made a trial of his skill with a renowned rope-dancer; in the other, and always in consequence of a challenge being made him, he acquired the art of horsemanship to a most extraordinary extent, and which certainly attracted attention, in the midst of first-rate performers, whose feats every evening attracted numerous spectators to the Circus of Franconi. Thus, those who delight in making contrasts may, on one side, picture to themselves Newton, the timid Newton, apprehensive of entering a carriage, lest it should be overturned, with his arms extended, and his two hands grasping the doors; and, on the other, his illustrious rival galloping erect upon two horses, with all the confidence of a professional equestrian.

In England, a physician, if he wishes not to lose the public confidence, must abstain from engaging in all scientific and literary research which appears to be foreign to the healing art. For long, Young sacrificed to this prejudice, and his writings appeared anonymously. This veil, it is true, was very thin; two contiguous letters of a certain Latin device successively served, in a regular order, as the signature for each memoir; but Young communicated the three Latin words to all his friends, domestic and foreign, without imposing secrecy. Besides, who could be ignorant that the illustrious author of the Theory of Interferences was the foreign secretary of the Royal Society of London; that he gave in the amphitheatre of the *Royal Institution* a general course of mathematical physics; that, associated with Sir H. Davy, he published the Journal of Science, &c. &c.; and besides, we must remark that the anonymous character was not regularly maintained, except for his lesser memoirs. On important occasions, as for example, when there appeared, in 1807, the two volumes in 4to of 800 or 900 pages each, where all the branches of natural philosophy are treated in a manner so new and profound, the *amour-propre* of the author obliterated the interests of the physician, and the name of Young, in large characters, replaced the two small italic letters, whose turn it was to have then appeared, and which, it must be confessed, would have cut rather a ridiculous figure on the title-page of this colossal work.

Dr Young then, either in London, or at Worthing, where he used to spend the summer months, had never an extensive practice. The public regarded him as over-wise! We must even state, that his lectures in medicine, those, for example, which he gave at St George's Hospital, were never popular. In explanation of this, it has been remarked, that the lectures were over-full, and too substantial, and that they exceeded the capacity of ordinary intelligences! But might not this want of success rather be attributed to the freeness, but seldom met, with which Young pointed out the inextricable difficulties which are encountered at every step in the study of the numerous disorders to which our feeble frame is subject?

Could it be thought, especially at a time when every one is anxious to gain his end speedily, and with little fatigue, that a lecturer on medicine could retain a numerous class, if he comcommenced with such words as we now quote from Dr Young: "No study is so complicated as that of physic. It surpasses the limits of human intelligence. Those physicians who rush forward, without attempting to comprehend what they see, are often as near the mark as those who engage in acute generalizations, based upon observations, in regard of which all analogy is defective." And if the professor, continuing the same strain, were to add: "In medical lotteries, the chance of him who possesses ten tickets, must evidently be greater than that of him who has only five." When they believed themselves engaged in a lottery, those of his auditors whom the former sentence had not dismissed, would be but little disposed to make great exertions to procure more tickets, or, to explain the thoughts of our learned associate, the greatest possible quantity of knowledge.

In spite of all his knowledge, and perhaps even on account of its immensity, Dr Young had no confidence at the bedside of a patient. At that moment, the troublesome effects which might eventually result from the action of the medicine, which was most clearly indicated, would crowd upon his mind,—would equal the favourable effects which might be anticipated from it, and would overwhelm him with indecision, perhaps not unnatural, but which the public interprets greatly to the prejudice of him who exhibits it. The same timidity is exhibited in all the works which Dr Young wrote on medical subjects. This man, so eminently remarkable for the boldness of his scientific views, now gave only simple catalogues of facts. He seemed scarcely convinced of the truth of his position, whether, when he attacked the celebrated Dr Radcliffe, whose only secret, in the most brilliant and successful practice, had been, as he himself declared, to employ the remedies which would check the symptoms; or, where he combated Dr Brown, who had found himself, he remarked, under the disagreeable necessity of recognizing, and that according to official documents of an hospital confided to the care of justly celebrated physicians, that, upon the whole, those fevers which are left to their natural course, are neither more severe, nor of longer continuance, than those which are treated with the most consummate art.

In the year 1818, Dr Young having been appointed Secretary of the Board of Longitude, almost entirely abandoned the practice of medicine, that he might devote himself to the minute superintendence of the celebrated periodical, known under the name of the *Nautical Almanac.* From this epoch, the Journal of the Royal Institution gave, every three months, numerous dissertations upon the most important problems of the nautical art, and on astronomy. A volume, entitled "*Illustrations of the Mécanique Céleste of Laplace*;" and a learned dissertation upon *the Tides*, would have fully attested that Dr Young did not consider the employment he had recently accepted as a sinecure. And, notwithstanding, this situation was to him a source of endless disgust. The Nautical Almanac had been, from its origin, a work exclusively intended for the service of the Navy. Some individuals insisted that it should also be made a complete astronomical ephemeris. The Board of Longitude, right or wrong, not having appeared particularly to favour the projected change, was speedily made the object of the most violent attacks. Journals of every colour, Whig and Tory, engaged in the discussion. There was no longer to be found in the union of Davy, Wollaston, Young, Herschel, Kater, and Pond, any thing else than an assemblage of individuals (I quote literally), "*who were under Beotian influence*;" the Nautical Almanac, formerly so famous, was become an object of shame to the English nation; if there was discovered a typographical error in it, as there ever has been, and ever will be, in any large collection of figures, the British Navy, from the smallest vessel to the largest three-decker, misled by the erroneous cipher, would assuredly be engulfed in the mighty deep.

It has been asserted that the principal promoter of these exaggerated follies never discovered so many grave errors in the Nautical Almanac, until after he had ineffectually endeavoured to get himself made a member of the Board of Longitude. I do not pledge myself for the accuracy of this statement; and at all events, I will not make myself the instrument of propagating the malicious remarks to which it gave rise. I ought not, in truth, to forget, that for many years the member of the Royal Society who is here pointed at, nobly consecrated a portion of his brilliant fortune to the advancement of science. This praiseworthy astronomer, like all learned men whose thoughts are concentrated upon a single object, had the misfortune, which I do not pretend to excuse, to measure through magnifying glasses the importance of those projects which originated with himself; but the point on which he is principally to be blamed is, that he did not perceive that the exaggerations of his disputes would be taken up in serious earnest; that he forgot that, in all times, and in all countries, there exists a great number of individuals who, inconsolable on account of their own insignificance, seize as their prey every opportunity of scandal, and, under the mask of the public welfare, are delighted to become the base Zoiluses of those among their cotemporaries whose fame proclaims their merit. At Rome, he who was appointed to insult the victor during his triumph, was nothing better than a slave; but in London, it was a member of the House of Commons who offered a cruel affront to these learned and illustrious individuals. An orator, who was previously celebrated for his prepossessions, but who had hitherto vented his spleen only on productions of French origin, attacked the most eminent men of England, and uttered against them, before Parliament, the most puerile accusations with laughable gravity. The ministry, whose eloquence was exercised for whole hours upon the privileges of rotten boroughs, did not utter a single word in favour of genius; and, finally, the Board of Longitude was suppressed without opposition. Next day, it is true, the necessities of a numerous Navy caused its voice imperatively to be heard, and one of the learned men who had been deprived of his office, the former secretary of the Board, Dr Young himself, was again called to resume his former labours. But it was a most inadequate reparation. The learned secretary, at least, should not have been separated from his colleagues; nor should this sensitive individual, rich in all the fruits of human intelligence, have been rated before the representatives of his country, like so much sugar, coffee, or pepper, in pounds, shillings, and pence.

The health of our foreign associate, which previously was in a very precarious condition, began, from this sad epoch, to decline with a fearful rapidity. The able physicians who attended him very soon lost all hope. Young himself had a strong conviction of his approaching end, and waited its approach with the greatest composure. Till his last hour he was unremittingly occupied with an Egyptian Dictionary then in the press, and which was not published till after his death. When his weakness no longer permitted him to rise, or to use a pen, he corrected the proof sheets by means of a pencil. One of the last acts of his life was to obtain the suppression of a pamphlet written with considerable talent, by a friendly hand, and directed against those who had assisted in destroying the Board of Longitude.

Young expired, surrounded by his family, by whom he was much beloved, on the 10th of May 1825, having scarcely reached the age of 56. He seemed to have died of ossification of the heart.

If I have not dwelt too long upon the interesting task which has been imposed upon me; and especially, if I have given that prominency which I wish, to the importance and the novelty of the admirable law of the Interference of Light, Young must now appear in your eyes as one of the most illustrious men of his day, of whom England may justly be proud. Your imaginations, anticipating my words, have already seen in the recital of the honours justly bestowed on the author of such a beautiful discovery, the peroration of this historical notice. These anticipations, however, I regret to say, will not be realized. The death of Young in his own country attracted but little regard. No title or civil honour was ever conferred upon him, and the doors of Westminster Abbey, so accessible to titled mediocrity, remained closed against the man of genius. The remains of Young were deposited at the village of Farnborough, in the quiet tomb of the family of his wife. The indifference of the British nation to labours which have added so much to its glory, is a very singular phenomenon, of which we may well be curious to know the causes.

I should fail in fairness, and should be the panegyrist rather than the historian, if I did not avow, that, in general, Dr Young did not sufficiently consult the intelligence of his readers, and that the greater number of his writings on science are faulty by being to a certain extent obscure. And still, the neglect in which for a long time they have been permitted to fall, has not been owing solely to this cause.

The exact sciences have an advantage over the works of art and imagination, which has often been pointed out. The truths of which they consist endure for ages, without suffering from the caprices of fashion, or from any depravation of taste. But it is also true, that, so soon as they rise to a certain point of elevation, the number of those who can judge respecting them becomes exceedingly limited. When Richelieu let loose against the great Corneille a crowd of men whom his merit had made furious, the Parisians repelled with indignation these hornets of the despotic cardinal, and applauded the poet. But this satisfaction is refused to the geometrician, the astronomer, and the philosopher, who cultivate the highest departments of science. Those who are competent to appreciate their labours, throughout the whole extent of Europe, never exceed the number of some eight or ten. If, then, we should suppose that these individuals were unjust, or indifferent, or it might be jealous, for I fear this may sometimes be witnessed, the public, reduced to the necessity of taking everything on their testimony, might be ignorant that d'Alembert had connected the great phenomena of the precession of the equinoxes with the principle of universal gravitation; that Lagrange had succeeded in assigning the physical cause of the libration of the moon; and that, since the researches of Laplace, the accelerated movement of this luminary is found to be connected with a particular change in the form of the orbit of the earth, &c. &c. The scientific journals, when they are conducted by men of known merit, thus acquire, in certain matters, an influence which often becomes highly injurious. It is thus, I think, we must qualify that which the *Edinburgh Review* has sometimes exercised.

Among the contributors of this celebrated Journal, there appeared, from the commencement, in the first rank, a young writer in whom the discoveries of Newton had excited the most ardent admiration. This feeling, so natural and fair, made him unfortunately disown whatever the doctrine of interferences contained of what was plausible, ingenious, and useful. The author of this theory had not always, perhaps, been careful to clothe his decisions, his decrees, and his criticisms in those polished terms from which merit can never suffer, and which, besides, were an imperative duty when he treated of the immortal author of "*Natural Philosophy.*" The penalty of retaliation was inflicted upon him with usury; the Edinburgh Review attacked the scholar, the writer, the geometrician, the experimentalist, with a vehemence and asperity of expression, almost without example in scientific discussion. The public is generally thrown upon its guard when it hears such impassioned language; but, on this occasion, it adopted, on the moment, the opinion of the reviewer, without however giving us reason to accuse it of levity. The reviewer, in truth, was not a beardless Aristarchus, whose commission was not justified by any previous study; many excellent papers, preserved in the Transactions of the Royal Society, testified to his mathematical knowledge, and had already assigned him a distinguished place among the philosophers to whom experimental optics was indebted; the English bar had proclaimed him one of its most distinguished ornaments; the Whigs in the House of Commons saw in him the sarcastic orator who, in their parliamentary struggles, was often the successful antagonist of Canning; it was, in fact, the future Chairman of the House of Lords, the late Lord Chancellor Brougham.*

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* The newspapers having sometimes done me the honour to state the many testimonies of kindness and friendship which Lord Brougham bestowed upon me in 1834, both in Scotland and in Paris, a few words in explanation appear here to be indispensable. The eloge of Dr Young was read in a public meeting of the *Academie des Sciences* on the 26th of November 1832; at that time I had never had any personal intercourse with the author of these articles in the *Edinburgh Review*; thus I can never, with the slightest propriety, be accused

And what could be opposed to the unjust criticism which proceeded from such a quarter? I am not ignorant that there are certain minds which derive constancy, from the conviction that they are right; and from the certainty that the truth will triumph, sooner or later; but I also know that we only act wisely in not counting too much on such exceptions.

Listen, for example, to Galileo himself, who, after his abjuration, exclaims in a whisper, "*E. pur si muove!*" And seek not in these immortal words any idea of the future, for they are the expression of the bitter vexation which the illustrious old man was suffering. Thus Young, too, in a small pamphlet in answer to the Edinburgh Review, shewed that he was greatly discouraged. The vivacity and vehemence of his expressions but ill disguise the feelings which oppressed him. In conclusion, we hasten to add that justice, complete justice, was ere long rendered to the illustrious philosopher! and for several years the whole world regarded him as one of the principal luminaries of our times. It was from France, and Young himself delighted to proclaim it, that the signal was given for this tardy act of justice. I shall add, that, at a much earlier period, when the doctrine of interferences had not yet made proselytes either in England or on the Continent, Young found in his own family one who comprehended it, and whose suffrages must have consoled him for the contempt of the public. This distinguished person whom I here point out to the commendation of all the philosophers of Europe, will, I trust, excuse me for my indiscretion.

In the year 1816, I passed over to England with my learned friend M. Gay-Lussac. Fresnel had then just entered in the most brilliant manner into the career of science by publishing his *Memoire sur la Diffraction*. This work, which, according to us, contained a *capital* experiment, irreconcileable with the New-

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of ingratitude. It may, however, be demanded, Could you not now, at the time of the publication of the work, entirely suppress everything which was connected with such an unfortunate discussion? Assuredly I could, and the idea, in truth, occurred to me; but I speedily dismissed it. I knew too well the high feeling of my illustrious friend, to fear that he would take offence in a question in which, I have the deepest conviction, that even the immense extent of his powers has not put him above the possibility of error. The homage which I thus pay to the noble character of my Lord Brougham, in now publishing, without any alteration, this paper of the eloge of Young, is, in my opinion, so significant, that I shall not attempt to say more upon the subject.

tonian theory of light, naturally became the first object of our communication with Dr Young. We were astonished at the numerous restrictions he put upon our commendations, and in the end he told us that the experiment about which we made so much ado was published in his work on Natural Philosophy as early as 1807. This assertion did not appear to us correct, and this rendered the discussion long and minute. Mrs Young was present, and did not appear to take any interest in the conversation; but, as we knew that the fear, however puerile, of passing for learned ladies—of being designated *Blue-Stockings*—made the English ladies very reserved in the presence of strangers, our want of politeness did not strike us till the moment Mrs Young rose up suddenly and left the room. We immediately offered our most urgent apologies to her husband, when Mrs Young returned, with an enormous quarto under her arm. It was the first volume of the "Natural Philosophy." She placed it on the table, opened it without saying a word at page 787, and pointed with her finger to a figure where the curved line of the diffracted bands, on which the discussion turned, appeared theoretically established.

I hope I shall be pardoned for these little details. Too many examples have almost accustomed the public to consider that neglect, injustice, persecution, and misery, are the natural rewards of those who laboriously consecrate their powers to the developement of the human mind. Let us not, then, forget to point out the exceptions when they occur. If we wish our youth to devote themselves with ardour to intellectual labour, let us shew them that there is a glory attached to great discoveries, which sometimes allies itself to somewhat of tranquillity and happiness. Let us even tear, if it be possible, from the history of science all those leaves which tarnish its brightness. Let us try to persuade ourselves that, in the dungeons of the Inquisition, a friendly voice whispered to *Galileo* some of those precious epithets which posterity applies to his memory; that within the thick walls of the Bastile, *Fréret* was apprized by the learned world of the glorious rank which was reserved for him amongst the scholars by whom France is honoured; that, before going to expire in the hospital, *Borelli* sometimes found in Rome a shelter from the storm, a little straw on which to rest his head; and, finally, that *Kepler*, the ler, never endured the agonies of hunger.

On the Powers and Use of Kater's Altitude and Azimuth Circle.

By Mr W. GALBRAITH.

THE importance of a small portable astronomical instrument, for the purpose of enabling scientific travellers to determine readily latitudes and time with sufficient precision to regulate chronometers, and thereby to find their longitude, has been long felt, and several expedients have been resorted to for the accomplishment of this end with various success. The small pocket sextant with an artificial horizon has been long used with considerable advantage. An instrument of this kind was generally employed by the celebrated traveller Mungo Park, on which several papers have been written, and the results of his observations form the subject of a memoir by M. D'Avezac, entitled, *Examen et rectification des positions determineés astronomiquement en Afrique par Mungo Park.* This was read to the Academy of Sciences at Paris on the 19th of August 1833, and of which an account is given in the *Connaisance des Tems pour* 1836.

The inconvenience attending the carriage and use of a mercurial horizon has been often complained of, and this induced the late Captain Kater to contrive a small altitude and azimuth circle, to be rectified by a spirit level permanently fixed to the instrument, which seems to answer the purpose admirably. The first of the kind the writer of these remarks had an opportunity of seeing, was one belonging to Captain Basil Hall, and made by T. C. Robinson, optician, Devonshire Street, Portland Place, London. The observations made with it were susceptible of very considerable precision, notwithstanding its moderate dimensions, the circles being, I believe, only about *three inches* in diameter.

" With respect to my little circle," says Captain Kater, in a letter to me of the 31st of June 1834, " there is no description of it published, and the state of my health is such, that I cannot undertake to do it, though I am convinced it is much wanted. My object in its construction was perfect portability and facility in its use, joined to a degree of accuracy sufficient for all the purposes of a scientific traveller. I therefore limited its diameter to *three inches*, and the reading to one minute, To shew you what my little circle will do, I send you eight single observations of the pole-star made on different evenings, (being the whole I have made here,) with three different instruments constructed for my friends. These observations might have all been made the same evening, and the difference of the mean from the truth is only 4.″83, my latitude being very nearly 51° 31′ 21″ N."—Captain Kater then resided at No. 2 York Gate, Regent's Park, London.

Observations referred to.

1. Observed latitude of York Gate,	51°	30′	54.″18	Error—26″.82
2.		31	15.96	— 4.04
3.		31	10.30	—10.70
4.		31	31.90	+ 10.90
5.		30	59.90	—21.10
6.		31	25.00	+ 4.00
7.		31	20.30	— 0.70
8.		31	31.80	+ 10.80
Mean, ...	51	31	16.17	
True latitude, ...	51	31	21.00	
Error, ...		—	4.83	

"But the first observation should, by rights, be rejected, having been made with an instrument scarcely finished, and much out of adjustment. In that case, the mean of the remaining seven would be only — 1″.69 from the truth. Such, then, are the results of an observer of great experience, and of undoubted veracity, and they shew in a remarkable manner the great precision which may be obtained by instruments of very moderate dimensions. In another letter previous to the above he remarks,—

"The circle you describe as of six inches diameter is too large for my construction. The size I recommend, and which I use, is only *three* inches diameter, and in the latest construction it has only a vertical circle, which can, however, be placed in the plane of two objects so as to take the angle between them. The whole is contained in a box *seven* inches long, *four and a half* wide, and *three* deep, so that it really deserves the name I originally gave it, of a pocket azimuth and altitude instrument." Such an instrument may therefore be, we think, strongly recommended to the notice of scientific travellers with perfect confi-

We have in our possession one of those of a much more powerful and perfect kind. The diameter is six inches, the telescopes magnify about twenty times, and there are three verniers to both the vertical and horizontal circles, each reading to the accuracy of ten seconds. The scale of the level attached to it shows *three* seconds, and a third, or at least a half of each division may be easily estimated. With this instrument I was inclined to believe, that observations, if carefully taken, might be obtained to a very great degree of accuracy. To insure all the precision possible, it was my practice to observe objects to the north and south of the zenith at nearly equal distances, as calculated most likely to destroy any casual errors in observing or reading. The general means are deduced from successive pairs to the north and south, and the final result is the latitude of my residence, No. 54 South Bridge, and as it is on the same parallel as the north front or side of Edinburgh College, it may be also considered the latitude of that much better known edifice.

Observations by the Writer in 1835.

1.	Polaris *north* and the Sun *south*,	55° 56′ 54″.9 N.	11.	α Aquilæ S. Polaris N.	55° 56′ 53″.8. N.
2.		56 57.0	12.	Sun,	56 55.2
3.		56 55.8	13.	α Aquilæ,	56 56.3
4.		56 55.7	14.	Sun,	56 55.8
5.		56 54.9	15.	Sun,	56 56.6
6.		56 57.0	16.	α Aquilæ,	56 57.2
7.		56 57.2	17.	α Aquilæ,	56 57.3
8.		56 55.5	18.	α Aquilæ,	56 57.3
9.		56 53.5	19.	α Pegasi,	56 57.8
10.		56 53.3	20.	α Pegasi,	56 58.05

The final latitude deduced is, therefore, 55° 56′ 58″.05 N. from 20 series to the north, and a like number to the south, and since there were from 6 to 18 readings to each, or about 12 at a mean, the whole 40 series of observations amounted to 480 readings, or 160 different observations, the circle being regularly reversed for each pair, at the same time recording the readings of the level. The observations of the pole-star were calculated by the aid of the table for that purpose at the end of the Nautical Almanac, while the others were reduced to the meridian in the usual manner by my tables.

What may be the accuracy of this final result I shall not take upon me at present to say, but I believe it to be within one or two seconds of the truth. Captain Kater was of opinion that with his smaller instrument he could, by a mean of five or six observations, come within 5″ of the truth, when made successively in the same evening on Polaris, or perhaps with more certainty on stars to the north and south of the zenith. From the unfavourable state of the weather during these few months past, it was difficult to obtain a favourable opportunity to try this effectually here, and this is the reason why Nos. 8 and 9 of the preceding series differ so much from the others. In fact, two sets made at that time should have been properly rejected, as they were made too hurriedly, and the bubble of the level was too near the extremity of the level to be confidently relied on. On the 28th of November last, five series, in which the circle was only once reversed each series, gave 55° 56′ 57″.5 N., from observations on the pole-star, which agrees very nearly with the general mean previously obtained. Whether so close a result could be always calculated upon, my experience with this circle does not enable me to decide. It is common even with instrument makers to estimate the accuracy of these too high. They frequently divide the value of one division of the vernier by the number of readings. In the present circle, which has three verniers each shewing ten seconds, if six readings be recorded, the instrument being once reversed, the probable error is taken at $\frac{10''}{6} = 1''\frac{2}{3}$, instead of $\frac{10''}{\sqrt{6}} = \frac{10''}{2.5} =$ 4″ nearly, or about three times greater. Still, however, when the observations are repeated a considerable number of times, the errors of reading, division, and pointing (*pointé* as the French call it) must be greatly diminished, if not completely destroyed, as we know from experience. Indeed, with a similar circle from a series of ten days' observations, John G. Kinnear, Esq. determined the obliquity of the ecliptic by observations taken in June 1834 to be 23° 27′ 41″.5, which exceeds the result that I derived from the Greenwich observations by 1″.2 only, and Bessels' by 2″.2 for January 1, 1834.

The deductions from these small but compact instruments are much more accurate, therefore, than from their size we had any reason to expect, and may be advantageously employed in many geodetical and astronomical operations with great success. Hence, the smaller sized ought to recommend themselves to scientific travellers and to medical officers attached to foreign tations, who are anxious to distinguish themselves in geogra- cal and astronomical researches.

Remarks on the Arrangement of the Natural Botanical Families. By Sir Edward Ffrench Bromhead, Bart. F. R. S. Lond. and Edin. Communicated by the Author.

It is instructive to review at intervals the progress of any science. This will usually be found to depend upon a succession of hypotheses, gradually approaching to the truth, each conducting, like the approximate root of an equation, to a closer approximation. The hypothesis is a nucleus round which facts accumulate, and, even under the most erroneous hypothesis, many facts are often truly arranged with respect to each other, and afford materials ready fashioned for a new structure. This, however, is generally forgotten, when the machinery has broken down with its accumulated weight; many facts, many valuable principles, and much partial truth in the old hypothesis, are overlooked in the rage for novelty, until some supposed discovery is found to have formed part of a previous long-forgotten system.

The science of botany is now in an interregnum; the method of Jussieu, as far as extends to the classification of the natural families, has broken down. It was greatly shaken by Brown, and subsequently by Decandolle and others, and has been finally demolished by Lindley in his work on the Natural Orders. At this period great caution is necessary, and a careful review of the successes and failures of the past.

Tournefort had found botany a mass of species; by establishing genera, and by acting on the principle, that "we must first form the generic group from nature, and afterwards endeavour to detect the generic difference;"—he left botany a new science. Linnæus found Tournefort's artificial classification of the genera broken down, and the species become unmanageable. Rivinus had thrown out the idea of a descriptive specific name in a single word; Linnæus saw the impossibility of this proposal, but established the present minute accuracy of the science by inventing the *trivial* specific name. He also followed the true principle in establishing his *natural* orders, adopting the idea of Magnol, that "all the parts of plants must be taken into account in judging of the affinities." Nor did this great man, in founding the natural system, neglect what had been done before; he searched preceding writers of every method, he found various assemblages already formed, and he adopted them. His research could not fail of success, if we consider that the force of development in different parts of the natural system may be thrown upon almost any class of organs, so that distinctions essential in one part may be unimportant in another, and every partial method may offer some natural combinations. Linnæus established here also the great principle, "that the natural assemblage must first be sought for, and the ordinal difference subsequently." So tenaciously did he hold to this, that Giseke informs us of his obstinate resistance to any thing like definition in the then state of the science, and he even encouraged an idea that the thing was impossible. Yet this great man ruined his own sketch, by falling into this very error, and allowing his great ingenuity to contrive descriptive instead of trivial names for the greater part of his families. He thus lost the glory of being named the father of the natural botanical system, and has in the same manner entailed theoretic names in zoology, which have long cramped the progress of Natural History.

That truly wonderful man Bernard Jussieu took up the subject, adopted Linnæus's sound orders, discovered new ones, and discovered the affinities of many of these orders to each other. He adopted *trivial* names, and, as might be expected, descriptive and differential characters rapidly presented themselves to Adanson, and perhaps to others. At last the immortal Antoine Laurent Jussieu presented this system with several new families, and the whole more or less accurately limited. He, however, yielded to the taste of the day, and, seizing on a character of great range, discovered by Gleditsch, who wished to modify Linnæus's artificial system by means of the adhesion of the stamens to the calyx, he applied the principle to the natural families, and thus threw them into artificial groups. This artificial arrangement of the natural orders has at last fallen to pieces, and the science is now a mass of confusion, presenting almost as many unarranged families and tribes as there were genera in the time of Tournefort.

The course to be pursued in this emergency is pointed out in the Linnean manuscripts, from which interesting extracts are subjoined to Sir James Smith's Grammar of Botany. Here Lin-

thrown his natural families into separate *natural* *groups*, unincumbered by hasty definition or theoretic nomenclature. Such is the true principle of the inductive philosophy, where analysis precedes synthesis, and definition follows knowledge.*

At this time we may observe a general tendency to artificial classification and theoretic names, especially among the many most able continental botanists.

We should, on the contrary, throw the families into natural groups, and *afterwards* endeavour to discover some differential characters for those groups, and for series of such groups. Whoever attempts arrangement through the discovery of a key (and every botanist has attempted it), will infallibly be disappointed. Neither will he be more successful in attempting to place families in a natural series by their individual relation; wherever he begins, he will find, after some steps, that he ends where he began, or that the families, by which the series is continued, are so utterly unconnected with what went before, that he resigns the attempt in despair. There is indeed much difficulty in the formation of natural groups, and a still greater difficulty in the arrangement of those groups with reference to each other. We cannot call a scheme satisfactory until the maximum of allied families is brought together, nor until each family is placed between two others to which it is more closely related than to any other. The materials for judging of these affinities are more copious than we could have expected: several of Jussieu's groups are tolerably natural, and in the larger groups there was free opportunity for placing the more nearly allied families in juxtaposition with a judgment passed upon their proximity; Linnaeus's views also remain unfettered by Jussieu's artificial method. The idea thrown out by Linnaeus, that families may be related as on a map, has also most happily given liberty to the opinions of writers of all schools, who felt the defects of existing methods, and who could not detect the arrangement of nature. This was in fact impossible amidst the confusion of orders not yet definitely established, or confounded with others of very different relation,—a difficulty partly arising from the unwillingness of botanists to increase the number of families, already too numerous for the existing arrangements.

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* See some excellent philosophical remarks by Professor Whewell.

Plants, no doubt, principally differ from each other in the degree and peculiar mode in which the parts are developed, and this without reference to the question whether they came separately from the hand of the Creator with their parts in a certain state of development, or whether all have arisen from the progress or regress of development in a few original types. Whether a system of radiation and branching be that of nature, or whether several series arise from distinct types, the families must probably, in the same stage of development of any structure, shew a certain parallelism or relation, which would, under our present indefinite views of affinity, sometimes cause them to be placed together. It is also conceivable, if the development should cease at any point, and a regress of structures occur in a modified degree and order, that a new set of relations would occur among families not properly contiguous. To meet these difficulties, it is clear that the opinion of no one botanist can be trusted, and that very strong evidence of relation cannot alone decide whether the relation be one of affinity or correspondence. Take for instance two parallel series,

a, *b*, *c*, *d*, &c.
a,' *b*,' *c*,' *d*,' &c.

and we may arrange these in apparent natural connection,

a, *a*,' *b*,' *b*, *c*, *c*', *d*,' *d*, &c.

though the order is unnatural, and such as never could form a foundation for drawing conclusions as to the physiological sequence. In an acute and accurate writer, I have found four families, from four distinct corresponding series, placed in juxtaposition, and the fact speaks for the sagacity of the writer under the circumstances of the science. But we are not wholly without a clew;—in the natural series *all the adjoining families are related* to each other; in the parallel series the relation is in a great measure limited to the families parallel, without strikingly extending to the contiguous families. A great source of error has been the endeavour to *force* together all the families which show relation; our course, on the contrary, should be to form groups of families *continuously* connected, throwing aside those which do not easily come in succession, for future inquiry, as being probably parallel or of accidental resemblance. The families so thrown aside will often most unexpectedly form themselves into natural groups, after cumbering many other series. If a doubt arise as to which of two groups a family belongs, we must collect the evidence of its relations, and if we find it related (according to the testimony of different botanists as to different families) to a great number of contiguous families, we must not hesitate to place it there, notwithstanding some strong opinions of its relation to a single family, or only a few in another part of the system. Hence we separate *Leguminosæ* without hesitation from *Rosaceæ.* We must not weigh the strong expressions of writers as to particular relations, in which they may be insensibly influenced by system, especially in enhancing relations required for the support of Jussieu's method. Every statement of relation should, however, be considered as evidence of relation; and we may consider in the same light those cases in which botanists have thought it necessary to take distinctions between two orders.

The authorities are, in this mode of investigation, to be numbered rather than weighed, are to be taken as a matter of average, and to derive value from mutual support, evidencing the relation of many families to many others, and not merely the relation of a single family to another. If the result should unexpectedly bring together families differing in the adhesion of the calyx to the stamens, ovary, &c., we must not forget that botanists have, in such cases, given the evidence against their own impressions. My object in the appended sketch has not been to follow my own judgment or that of any writer whatsoever, but to bring together the greatest possible number of admitted affinities, and, if possible, in continuous succession. Isolated and unconnected resemblances, often accidental or unimportant, and among orders scattered remotely from each other, must be expected, where they want the weight of joint, combined, accumulated evidence. The process adopted may no doubt misplace some families, but there is reason to believe that the great majority will be in their true neighbourhood.

I have not at all satisfied myself in fixing the arrangement of the families within each group, which is a matter not important at present; and in many cases the limits of the group are ambiguous, and must so continue until a physiological key shall be found from a careful examination of the whole. The true natural scheme remains to be collected by induction, from a comparison of the arrangement of species in genera, genera in families, and families in groups, alliances, and races.

The Parallelisms and Correspondences closely examined may hereafter lead to making the limitation of the groups mutually more definite, and may suggest points of structure.

The Table is to be considered as a sketch for future correction, in which certain families and groups may change places, though the whole may present something like a fixed basis to work upon, and definite tangible limits of inquiry.

There is a general tendency in the groups to form themselves into a re-entering or fusiform series: the same tendency is shewn in sets of groups, and in sets among each other. This causes the greatest difficulty in arrangement, as families, *prima facie* adjoining, may form the terminating points of the group; and when two corresponding groups lie near, the combined effect of relation and affinity is most embarrassing. If the reader is surprised at finding any two families in separate groups, let him apply the groups to each other; and a correspondence will probably appear on comparing the preceding and succeeding groups of each series progressively and regressively.

Botanists ought, I think, to lean to the division of families, as facilitating their relative arrangement; the formation of groups will, in many cases, be a useful check to improper compression or division. When orders have been divided, much error has arisen from authors continuing to declare an affinity to the order under its old name, though that affinity may be confined exclusively to the separated order.

The mode of investigation here followed has, of course, often failed, the evidence for different situations being equally balanced. This has been the case among single families, and in the parallel of the Amentaceous families, and also among the Endogenous families, where authors writing at random have given full scope to artificial arrangement, and compared each family with almost every other. So also from circulation, parallelism, and correspondence, it has been impossible, without something arbitrary, to separate some others.

Sketch of an Arrangement of the Botanical Families in Natural Groups, Alliances, and Races.

1. Families having any material similarity of structure are in that respect said to have a *Relation.*

2. Related Families lying in the same neighbourhood are said to have an *Affinity.*

3. Families which touch or pass into each other are said to be *Adjacent,* or in *Juxtaposition.*

4. The Numerical superiority of related families, which, (with the aid of juxtaposition and progress of structure) determines the place of each family, and distinguishes the relation of Affinity from those of Parallelism and Correspondence [see 9, 11,] is called *Joint-affinity.*

5. A *Group* is a collection of Families, having an affinity, and is named from some family contained in it;—Ex. The Orchideous Group.

6. The two great Botanical Divisions are named The Chenopodeous and Thymelæaceous *Races.*

7. When the first and last families of a Group, or when the initial and terminating parts of a series of Groups or of the two Botanical Races, correspond and seem to pass into each other, they are called *Re-entering* Groups, &c., and are said to *Circulate*:—Ex. The Chenopodeous, Boragineous, Geraniaceous, Passifloreous, Nymphæaceous Groups.

8. An *Alliance* consists of a Circulating Series of Groups, and is named from the denominating family of one of the Groups:—Ex. Gramineous, Orchideous, Boragineous Alliances.—Those set out in the Table are generally to be considered as artificial divisions provisionally adopted.

9. The Groups and Alliances of the two Races, in the same numerical order from the initial Group, are said to be *Parallel.*

10. As certain Acotyledonous, Gymnospermous, Amentaceous, Apocarpellous, Apetalous, Monopetalous, Gynobasic, Albuminose, Monocotyledonous, and other peculiar *Structures,* are usually in the same parallel, and in the same stage of developement, they are likely to be treated of together, and may be named after the compound parallel Families:—Ex. The Endogenous Families would be The Orchideo-Gramineous Alliances.

11. Successions of Groups related to other successions direct or reverted, in the same or in separate Races, are said to *Correspond.*

12. Characters which give the structure of the organs in order, are said to be *Descriptive*; Characters (positive or negative) which distinguish one Group, &c. from another, are said to be *Differential.* Properties which may in certain cases be substituted for each other in a character, are said to be *Equivalents.*—Ex. Albumen and a Macropodal Embryo, &c.

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† Indicates that the Family may be compound, which is not of moment, where the sections pass into each other.

( ) Indicates that the evidence for the station is more conflicting than usual.

Suborders are inserted in the table, to show the transitions, or to show the conditions which remain unsatisfied.

SKETCH OF AN ARRANGEMENT OF THE

Fucoideae, florideae, ULVACEAE, nostochineae, diatomeae,

†Confervoideae, CHARACEAE, equisetaceae,
Ophioglosseae, danæaceae, OSMUNDACEAE, gleicheneae, polypodiaceae,
CYCADEAE, ephedreae, casuarineae,

MYRICEÆ, plataneae, (†)artocarpeae, ulmaceae, (fothergilleae,) (empetreae,)
Stilagineae, URTICEAE, cannabineae, datisceae, lacistemeae,
Chlorantheae, garryaceae, (henslovieae), gnetaceae, PIPERACEAE, saurureae, podostemeae,

Ceratophylleae, hippurideae, callitrichineae, HALORAGEAE, hydrocaryes,
Circæeae, †ONAGRARIAE, montinieae, †salicariae, rhizophoreae, vochyaceae, terminalieæ, †combreteae,
Memecyleae, melastomaceae, alangieae, lecythideae, †MYRTACEÆ—granateae,

Pomaceae, amygdaleae, neillieae, chrysobalaneae, sanguisorbeae, quillajeae, spiræeae, ROSACEÆ, dryadeae,

SAXIFRAGEÆ, hydrangeaceae, cunonieae, bauereae, philadelpheae, (galacinae,) escallonieae, (brunieae,)
Grossulaceae, gronoviaceae, CUCURBITACEAE, begoniaceae, loaseae, cacteae, rhipsalideae,
Portulaceae, tamariscineae, reaumurieae, fouquiereae, nitrarieae, FICOIDEAE, neuradeae, surianeae, crassuleae, (†)illecebreae,

Scleranthеae, amarantaceae, CHENOPODEAE, phytolacceae, petiveraceae, polygoneae, nyctagineae,

Staticineae, plumbagineae, POLEMONIACEAE, cobæaceae, hydroleaceae,

Hydrophylleae, BORAGINEAE, heliotropiceae, ehretiaceae, cordiaceae,

Nolaneae, verbasceae, scrophularieae, digitaleae, salpiglossideae, (†)SOLANEAE, convolvuleae, cuscuteae, retzieæ,

Menyantheae, GENTIANEAE, spigeliaceae,
†APOCYNEÆ, asclepiadeae, strychneae, potalieae, loganieae, lygodysodeaceae,
CINCHONACEAE, opercularineae, stellatae, lonicereae, sambucineae, umbelliferae,

Araliaceae, adoxeae, lorantheae, (†)hamamelideae, corneae, HEDERACEAE, vites, leeaceae,

GERANIACEÆ, tropæoleae, limnantheae, [hydrocereae,] balsamineae, †oxalideae,

Lineae, vivianieae, sileneae, (†)alsineae, elatineae, CISTINEAE, ((†)prockiaceae, (†)flacourtieae,)

Cappavideae, resedaceae, cleomeae, CRUCIFERAE, fumariaceae, papaveraceae,
NYMPHÆACEAE, nelumboneae, cephaloteae, hydropeltideae, podophylleae, pæoniaceae,
Cimicifugeæ, clematideae, †ranunculaceae, SARRACENIEAE, aristolochieae, nepentheae, (†)cytineae,

Pistiaceae, †hydrocharideae, ALISMACEAE, butomeae, pontedereae,

Commelineae, philydreae, xyrideae, restiaceae, desvauxieae, cyperaceae, GRAMINEAE, palmae,

Cyclantheae, pandaneae, TYPHACEAE, acorineae, symplocarpeae, pothoinae, †aroideae, juncagineae,

NATURAL BOTANICAL FAMILIES.

†† Fungi, . . . hypoxyleae, USNEACEÆ, lecanorineae, calycieae,

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Ricciadeae, †hepaticae, jungermanniaceae, andræaceae, MUSCI,
Salvinieae, marsileaceae, isoëteae, LYCOPODIACEAE, lepidodendreae,
(†) Taxineae, CUPRESSINEAE, araucarieae, †abietinae,

Liquidambarinae, salicineae, BETULINEAE, carpineae, †corylaceae, juglandeae,
SUMACHINEAE, anacardieae, spondiaceae, burseraceae, rhamneae, (phytocreneae),
Coriarieae, (†)euphorbiaceae, stackhouseae, CELASTRINEAE, staphyleaceae, hippocrateaceae, erythroxyleae,

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Malpighiaceae, acerineae, HIPPOÇASTANEAE, millingtonieæ, sapindaceae, rhizoboleae,
Guttiferae, marcgraaviaceae, HYPERICINEAE, carpodonteae, camelliaceae, †ternstromiaceae,

Chlenaceae, humiriaceae, (hugoniaceæ,) meliaceae, cedreleae, AURANTIAÇEAE, amyrideae,

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Cannaraceae, mimoseae, swartzieae, detarieae, PAPILIONACEAE, geoffreae, cæsalpinieae,

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Moringeae, droseraceae, (parnassieae,) sauvageae, (frankeniaceae,) alsodineæ, †VIOLACEAE,
PASSIFLOREAE, malesherbieae, turneraceae, papayaceae, belvisieae, (patrisieae,) (paropsieae,),
Homalineae, samydeae, (hymenantheræ, polygaleae,) (tremandreae,) CHAILLETIACEAE, aquilarineae

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(Penæaceae,) (proteaceae,) elæagneae, THYMELAEAE, nyssaceae, †santalaceae, exocarpeae, olacineae,

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Oleaceae, fraxineae, JASMINEAE, (stilbineæ,) columellieae, aragoaceae, bignonieae, pedalineae, didymocarpeae,
Acantheae, selagineae, myoporeae, †verbeneae, LABIATAE, buddlejeae, buchnereae, veroniceae, sibthorpieae, teedieae, gratioleae, lentibulariae,
Gesnereae, calceolarieae, hemimereae, ANTIRRHINEÆ, cheloneae, gerardieae, euphrasieae, rhinantheae, orobancheae,

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Monotropeae, epacrideae, †ERICEAE, vaccinieae,
CAMPANULACEAE, lobeliaceae, stylidieae, goodenoviae, scaevoleae, brunoniaceae,
Corymbiferæ, †compositae, valerianeae, calycereae, DIPSACEAE, globularineae, plantagineae, littorellideae,

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†Primuleae, †MYRSINEAE, aegicereae, sapoteae, styraceae, symploceae, halesieae, †ebeneae, ilicineae, brexieae, pittosporeae,
†Zygophylleae, RUTACEÆ, diosmeae, cusparieae, pteleaceae, xanthoxyleae, ailantheæ, simarubaceae, ochnaceae,
Dipterocarpeae, lophireae, elaeocarpeae, (†)tiliaceae, bombaceae, MALVACEAE, sterculiaceae, †byttneriaceae,

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Myristiceae, hernandieae, (illigereae.) gyrocarpeae, cassytheae, †LAURINEAE,
Atherospermeae, monimieae, CALYCANTHEÆ, wintereae, magnoliaceae, dilleniaceae,
Schizandreae, anonaceae. (†)berberideae, MENISPERMEAE,

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Dioscoreae, tameae, SMILACEAE, roxburghieae, ††asphodeleae, (†)junceae, gilliesieae, alliaceae, erythronieae,
Melanthaceae, irideae, apostasieae, cypripedieae, ORCHIDEÆ, scitamineae, marantaceae, musaceae, amaryllideae,
(†)Liliaceae, wachendorfieae, †BROMELIEAE, barbacenieae, hypoxideae, hæmodoreae, burmannieae, tacceae, (†)balanophoreae,

It was my intention to have subjoined, as extracts from the evidence upon which the Table is founded, the opinions of botanists on the relation of each family to others in the same or adjoining groups, which is interesting historically, and sometimes of value as shewing the structure on which the opinion was formed; but I have found the accumulation too great. The English reader will see the greater part of the whole in Dr Lindley's Natural System, admirably set out, and abounding with original views. He will also find valuable and *original* views of relation in the publications of Dr Brown and Mr D. Don, in Mr Arnott's article on Botany in the *Encyclopædia Britannica*, and in Richard often translated.

I have intentionally kept back, for the present, any attempts at characters general or partial, such being quite premature, until some outline of arrangement has been recognized by botanists. Materials are not wanting, many groups having been already treated as distinct families by able writers, such as the Cupressineous, Rosaceous, Boragineous, Gentianeous, Geraniaceous, Malvaceous, Osmundaceous, Rutaceous, Ericeous, Campanulaceous, &c. and also other groups, with slight modification.

Finally, I may mention the Nixus of Dr Lindley, which would have altogether prevented this attempt, had I seen it sooner: here the botanist will find most ample and most original materials for characterising those or any other groups pretending to be natural. To this distinguished botanist I am indebted for pointing out some erroneous deviations from his published views, in the case of Spigeliaceæ, Araliaceæ, Euphorbiaceæ, Granateæ, Empetreæ, Dioscoreæ, Ternstromiaceæ, Myrsineæ, Papayaceæ, Violaceæ, Terminalieæ; also for the places of Begoniaceæ, near Cucurbitaceæ; Elatineæ, near Cistineæ; Stackhouseæ, near Euphorbiaceæ; Stilagineæ, near Urticeæ; Limnantheæ, near Tropæoleæ; and for references to sources of information.

Thurlby Hall, Lincoln,
October 1835.

Abstract of the Memoirs of JOHN NAPIER *of Merchiston.* By M. BIOT.[*] With Notes by the Translator.

THE following translation of M. Biot's very able and interesting paper, of which the title is given below, will be acceptable to our readers. The history of the illustrious Inventor of Logarithms had remained too long unrecorded, and this early and important attention which Mr Napier's work has met with on the Continent, is as flattering to the science of Scotland as it must be gratifying to the author of those memoirs.

FIRST ARTICLE.—Montaigne, in his chapter of Proper Names, puts the question, To whom belongs the honour of so many victories, to Guesquin, Glesquin, or Géaquin, seeing thus variously the name of that famous constable is written. If intellectual conquests and the glory of arms admit of any analogy, and we shall not pause to consider which would suffer by the comparison, the same question might be put with regard to him who, simply by an arithmetical invention, increased, as it were an hundred fold, the scientific life of Kepler, Halley, Bradley, Mayer, Lacaille, Piazze, Delambre,—prolonged that of La Place, nay of Newton himself, and still indefinitely continues the like miracle for all whose zeal, if it be not genius, prompts them to emulate those great men in the application of mathematics to the phenomena of nature. For to this hour we cannot say with certainty whether that puissant instrument, the logarithms, be due to Neper, Napeir, or Napier.[†]

Even at the time it was first made public, in the year 1614, the author was so little known beyond his own country, that Kepler, who afterwards embraced and adopted that invention enthusiastically, as a miraculous aid to his Rudolphine Tables,—Kepler knew nothing of the matter until the year 1617, and then his knowledge of it was but imperfect, having merely seen Napier's work at Prague, when he had not an opportunity of studying the contents; so that, unhappily for himself, he did not properly appreciate the invention, as is evident from the letter he wrote, about that epoch, in which the author of the logarithms is simply designated as "*Scotus Baro, cujus nomen mihi excidit,*[*] a Scottish Baron, whose name has escaped me."[*] One year later, however, an abridged, and perhaps plainer exposition of the discovery having accidentally fallen under his observation, "I comprehended (he exclaims) the nature of his work, and scarcely had I essayed a single example of the process, when, to my great joy, I became sensible that he had infinitely surpassed all the attempts at abbreviation which I myself for a length of time had been labouring to effect." He set one of his pupils instantly to work, made him calculate logarithmic tables by Napier's method, availed himself gratefully of their assistance to complete the calculations of his Rudolphine Tables, which hitherto had cost him unimaginable labour, and even changing the whole plan of those tables, though they had been long in hand, he courageously gave them that new form by which they are adapted to the usage of logarithms. Upon what accidents depends the progress of human knowledge! The Rudolphine Tables appeared in 1627, six years only before the death of Kepler. Who can say that, without the unforeseen succour of logarithms, he would have had time to complete those tables; and yet they were destined to become the basis of all our ulterior knowledge of the system of the world. For being established, for each planet, on the conditions of elliptical motion, and for the mutual ratios of the orbits in terms of the proportionality of the squares of the periodic times to the cubes of the mean distances, their invariable accordance with the actual state of the heavens offers perpetually the essence, as well as the proof, of those great astronomical laws justly called the *laws of Kepler.* From these it was that Newton deduced, mechanically, his law of the central force proportional to the masses, and reciprocal to the square of the distance, which is purely a concentration of the former. But if the general conditions of the planetary movements had not been previously known and demonstrated, Newton would not have been enabled to remount to the law of gravitation. So that without the invention of logarithms, which in some sense rendered the life of Kepler long enough to achieve his task, perhaps universal gravitation would have been yet to be discovered. That revolution, so fortunate in Kepler's tables and calculations, has been described and celebrated by himself in a letter to Napier, dated 28th July 1619, which he placed at the commencement of his Ephemerides for the year 1620; but this important illustration of the history of letters had become so rare that neither Montucla nor Delambre were aware of its existence. Happily, however, the Bodleian Library at Oxford possessed a copy, of which the author of the present work obtained an accurate transcript, which he has inserted in his text; and from that we have derived the details given above. Napier never received this letter, which would have overwhelmed him with joy. He had been dead for two years, since 14th April 1617, and Kepler knew it not; so difficult and slow was the communication between scientific men, in those times of wars and storms, occasioned by the shock of political interests, and by the change of religion.

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[*] *Mémoire sur J. Napier de Merchiston, contenant sa Généalogie, sa Vie, le Tableau des temps où il a vécu, et une histoire de l'invention des Logarithmes. Par Mark Napier.—Extrait en trois articles, par M. Biot.—Tiré du Journal des Savants, année* 1835.

[†] We find these distinct varieties in the biography we are abstracting. A letter from the Inventor of Logarithms to his father, quoted at page 150, is signed *Neper*. His dedication of the exposition of the Apocalypse, addressed to the King of Scotland, James VI., is signed *Napeir*, p. 172. His testament, quoted p. 431, is signed *Naipper*; lastly, his own biographer calls him throughout *Napier*.—*M. Biot.*

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[*] This remarkable passage is to be found in a letter, written by Kepler to his friend Schikkart, 11th March 1618, as follows: "Extitit Scotus Baro, cujus nomen mihi excidit, qui præclari quid præstitit, necessitate omni multiplicationum et divisionum in meras additiones et substractiones commutatâ; nec sinibus utitur. Attamen opus est ipsi tangentium canone; et varietas, crebritas, difficultasque additionum, substractionumque alicubi laborem multiplicandi et dividendi superat." (Epist. ad G. Keplerum, Lipsiæ, 1718, in fol. p. 672). The last clause of this sentence proves, as we have said, that, *at that first inspection*, Kepler had ill appreciated the Neperien method. The objection *attamen opus est ipsi tangentium canone*, appears to us to require some explication; and it is this,—that in the original publication of his discovery, in 1614, of which we have before us a copy belonging to the library of M. Walkanaer, Napier does not furnish a special table for the logarithms of natural numbers, but only for the sines, cosines and tangents of arcs. Thus when it is required to find the logarithm of a given number, he supposes it considered as a natural sine, if it be comprehended betwixt 0 and 1, and as a natural tangent, without those limits. In the first case, the logarithm sought is found directly among those of Napier's table of sines; in the second case, it is necessary to begin by seeking, in a table of natural tangents, *the arc* which corresponds to the given number, and with that arc Napier's table gives the logarithm.—*M. Biot.*

If such was the state of continental Europe, that of Scotland was still worse. The inhabitants of its Highlands, divided into semi-barbarous tribes, spent their lives amid a succession of wars and brigandage, perpetuated by the interminable quarrels of their savage chiefs. The regal authority, powerless to set at rest their hereditary conflicts, was regarded by such ambitious vassals as nothing else than an instrument of domination and of fortune, which each struggled to obtain for himself by becoming the most formidable. To this we must add the first propagation of those new ideas of religious reformation, entertained by a few from sincere conviction, by many from interested motives, or fanaticism; while, on the other hand, opinions and interests diametrically opposed to these, conspired with equal violence to exclude them. In such times, and in such a country, it may readily be supposed, that after the lapse of two centuries, no traces should exist of the first years of an infant, however distinguished as a man in the unexplored paths of abstract science. Hence, notwithstanding the most indefatigable research, his Scotch biographer, with the exception of some vague and unimportant indications, has not been able to throw light upon the education of young Napier. To fill this void, he launches into a course of interminable digressions, relating, for example, the biography, more or less obscured by time, of six or seven Napiers of Merchiston, the lineal predecessors of the inventor of logarithms, their fortunes, their alliances, the transactions political, commercial, military or civil, in which they had been actors,* and as these ancestors, according to the fashion of Scotland, are discovered to be related, in one degree or other, to personages who then played a conspicuous part, (among the rest the famous Bothwell, whose espousals of Mary Stuart were none of the most gentle,) our author gives at full length the history of Mary, of Bothwell, of Darnley, with occasional digressions, by way of by-play, in which figure Louis XI., Charles-le-Téméraire, and even certain characters, of an association yet more bizarre in such a subject, namely, the page Quentin Durward, and the Abbot of Misrule. Then, as it appears that young Napier passed some years at the University of St Andrew's, we are presented with its history, or rather the history of the most celebrated persons of the times who were reared at that University. From all this we can gather no more respecting the inventor of logarithms, than that he sprung from an ancient, wealthy, and distinguished family, who had unavoidably taken a part, though reserved and prudent, in the political affairs of the times. Born in the Castle of Merchiston, in the year 1550, Napier was entered as a student at St Andrew's in 1563, and quitted it a few years afterwards for the Continent, where probably he went to complete his education, a practice then very prevalent among Scotchmen of distinguished rank. Returning to Merchiston in 1571, he married in the year following, and immuring himself in that retreat, divided the rest of his days between two principal occupations, the management of his family domains, in which his father had invested him, and his studies, theological and mathematical, for which he appears to have had an equal predilection. But with all his inclination for repose, too frequently was he compelled to quit that asylum, sometimes that he might escape a siege, and sometimes to take that part in the political transactions of his day, which his position in society and his religious opinions suggested. There, by means of the numerous and unquestionable documents which his biographer has collected, we can trace his steps; and our contemplation of the particular system of ideas, or the peculiar turn of mind which he brought upon the stage of mun-

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* It may be doubted if a French philosopher, however accomplished, can fairly appreciate this portion of Mr Napier's work. It was not possible to furnish a pure biography, in the proper view of such compositions, of the inventor of logarithms, whose isolated habits had left scarcely any domestic traces of the man. But it happened that his family charter-chest contained some curious documents connecting the history of his lineage, for centuries, in a remarkable manner with the history of Scotland, and with the personal fortunes of the ill-fated House of Stuart. His biographer, therefore, independently of Napier's own history, has compiled with labour and research a domestic collection of Scotch historical antiquities, which will be appreciated by a certain class of readers in Scotland, who may not be, perhaps, so much interested in the scientific history of Napier himself. The miserable state of the records in Scotland created a school of which Lord Hailes is the illustrious founder, by means of whose minute researches materials for a complete history of Scotland are still being collected. This cannot be so well under-

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stood by a foreign philosopher; and we must add, that the first chapter of M. Biot's abstract, which gives such popular interest to his paper, appears to have been entirely suggested by the historical portion of Mr Napier's work.—*Translator.*

dane affairs, will not be unavailing to perfect that philosophical point of view under which we must regard him.

It was the period when the crisis of the Reformation agitated Scotland with peculiar violence. James VI., afterwards James I. of England, then reigned over that distracted country; a Prince habitually feeble, yet not incapable of displaying a certain degree of firmness, by no means devoid of knowledge, we should say of erudition, especially upon the subject of religion, yet rarely failing to render himself ridiculous by the clumsy self-esteem with which he paraded his learning, tormented by the incessant revolts of his unruly vassals, by the demands, becoming daily more audacious, of the reforming party, whose puritanism watched with suspicion his indulgence, nay his partiality, for the Catholics; disquieted in the extreme by the ambitious Elizabeth, who was perpetually setting snares for him, ill brooking to see in him her immediate and inevitable successor, sprung from the very blood which her jealousy as a woman, and her policy as a queen, had caused to flow. In this state of frequent perils and annoyances, the poor King of Scots kept beating about like a ship in a storm, most anxiously on the look-out for fair weather; and it is amidst these struggles betwixt puritanism and royalty that the Baron of Merchiston appears upon the scene. He took part with the Presbyterian Synods then pursuing the King with indefatigable audacity, and pressing upon him their fanatical demands against the Catholics, whom, according to their opinions, his Majesty was not persecuting with sufficient zeal. Napier was a member of the Synod of Fife, the most violent of all the synods. He was one of the deputies whom that synod, and afterwards the General Assembly at Edinburgh, selected to carry to James their solemn deliberation, in which they declared, " that the principal and chief enemies, the Earls of Huntly, Angus, &c. (here follows a list which includes the father-in-law of Napier himself) have, by their idolatry, heresy, blasphemy, apostasy, perjury, and professed enmity against the Kirk, and true religion of Jesus Christ within this realm, *ipso facto* cut off themselves from Christ and his kirk, and so become most worthy to be declared excommunicated and cut off from the fellowship of Christ and his kirk, and to be given over to the hands of Satan, whose slaves they are, that they may learn, if it so please God, not to blaspheme Christ or his Gospel," &c. (p. 161). Such were the saintly pretensions of the pious Presbyterians! Observe, that excommunication involved the confiscation of the property of the impious, which by right devolved upon the King, for the purpose of distribution among the "Saints of God," as these good people then styled themselves. In vain the poor King made the most vigorous efforts, and the wisest, to smother these odious proclamations, and to prevent their reaching himself. He was constrained to admit into his presence the Commissioners of the General Assembly. It is curious to perceive, even yet, in the present age, the hereditary effect of the ancient puritanical exaltation, upon the mind of the Scotch biographer. He is in ecstasy at the grandeur of the part assigned to Napier, in these fanatical proceedings. "Our philosopher (says he, p. 162.) must have been particularly conspicuous at this convention, which confirmed the excommunication of his own father-in-law." (This was the father of his second wife, for he had lost the first in 1579.) Then pursuing, without hesitation, the consequences of that act, "if the family of Napier (he adds) attended their parish-church on the day appointed, they must have heard their grandfather doomed to exclusion from the social comforts of life, and the blessings of the Church." A little further on, he delights in picturing the subduing effect upon poor James, of the aspect of the "*majestic* Napier," with his "serene presence, thoughtful eye, and ample beard, rarely seen within the royal circle." Has he not discovered a merit very essential to be bestowed upon the Inventor of Logarithms, and withal very closely connected with that discovery? But, it may be said, why, then, do you so pointedly quote these details? I do so, because, in the evident intention of the biographer, they have an object, and one, in my opinion, opposed to the spirit of the sciences and of sound philosophy. That object is to represent the Inventor of the Logarithms as a light of the Protestant Presbyterian Church, as the greatest theologian of his times, as principally a theologian; and that, too, in order to bolster up religious belief, by scientific discovery; and under cover of that pretext, to tax our credulity with demands at which common sense revolts, and which, thank God, do not belong to the present age.

Unquestionably Napier was a theologian, a learned theologian, and unquestionably, also, his religious belief was perfectly sincere. His moral character is entitled to this concession. The importance of the arithmetical invention, which we owe to him, is also very great; already we have established that fact by indisputable consequences, and we shall presently have another occasion to be more precise in our specification of its merits, when we come to characterize the discovery. But does it follow that, with the arithmetic, we must accept the theology; or must we of necessity, for example, like his Scotch biographer, pronounce Napier's commentary on the Apocalypse admirable?* because he, too, before the time of Newton, wrote a similar commentary, where, in like manner, he undertakes to establish, by force of reason, that the Pope is Antichrist, and Christian Rome the Whore of Babylon. As for the rest, it was not new at this epoch, being equally the favourite disputatious theme of that thundering Presbyterian preacher Knox (who called the charming Mary Stuart a Jezebel); and did not King James VI. himself, in like manner, exert his theological learning to prove that point? It was then a current idea. But that which is peculiar to Napier, in this arduous controversy, is the having introduced a form of argumentation altogether mathematical,—a march of discussion logically knit, placing as a preliminary in the very front of his treatise, a table of *Postulata*, to support him in the interpretation of the Divine symbols; which *Postulata* themselves he takes the greatest possible pains to establish upon a host of learned authorities. I have not the temerity to enter the lists with such gifted persons, nor even to examine too punctiliously whether the number, already not inconsiderable, of elements admitted as fundamental positions, augmented, in the course of the discussion, with a good many other hypotheses, do not weaken very much, humanly speaking, the mathematical probability of the final deductions. I admit it all, if you will, confessing myself unequal to the dispute; and thus am I, of logical necessity, constrained to admit, with the Inventor of the Logarithms, that the Pope is certainly Antichrist; that he is also Gog, as the Emperor of the Turks is Magog, and his soldiers the locusts of the Apocalypse; and, besides all this, that there were two-and-twenty Popes, horrible necromancers, who sold themselves everlastingly to the devil, that they might become Popes; seeing that all this is equally established in Napier's book, at the places I have noted below.*

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* I could not find in Paris the original edition of that work, but only the French translation, published at Rochelle in 1602, under this title; "Ouverture de tous les secrets de *l'Apocalypse de Saint Jean*, par deux traités; l'un recherchans et prouvant la vraie interpretation d'icelle; l'autre appliquant aü texte cette interpretation paraphrastiquement et historiquement. Par Jean Napeir, c'est-a-dire, Non Pareil, sieur de Merchiston, revue par lui-mesme, et mise en Françaisе par George Thomson, Escossois." The revisal of this translation by Napier himself, gives it nearly the authenticity of the original edition; and, indeed, in the comparisons which I have had an opportunity of making with the original passages quoted by his Scotch biographer, it appears to be a very faithful translation.—M. Biot.

But, among all these conclusions, there is one which ought to be equally indubitable, and which, by its logical connexion with the others, obviously communicates to them its own character of infallibility. It is, that, according to the fourteenth Naperien proposition, "*the day of judgment* ought to arrive between the years of Jesus Christ 1688 and 1700; and hence, according to the tenth proposition, the world will come to an end rather before than after the year 1786." That is a consequence of which, it is true, I cannot dispute the necessity, as flowing logically from the premises; but I must confess, that, to my mind, it appears difficult to admit, and because it produces the same effect upon other simple souls is the reason, perhaps, why Napier's Commentaries on the Apocalypse is not in the present day read so frequently as one would desire, a neglect of which his biographer also complains. Newton too, as is well known, wrote a commentary on the Apocalypse; but he had not the hardihood to attempt so comprehensive a task as his Scotch predecessor has done. "The folly of former interpreters," says he (*folly*, that is a hard word), "was the aspiring to predict times and events by their prophecies, as if God had intended to make prophets of them." So Newton limits himself to the explanation of the past, and the greater number of those who have studied his work appear to have found even that no very easy task.

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* M. Biot here quotes some instances from Napier's Commentaries.

In giving an account of the commentary by Newton, in the *Biographie Universelle,* I had expressed some doubt as to the conclusion at which Newton arrived, that the eleventh horn of Daniel indicated the Church of Rome. Dr Brewster, in a work of the same kind (I mean of the same kind as my own), printed at London in 1832, has taxed me severely for my aptitude to doubt; he has affirmed, that that interpretation of the eleventh horn, as well as others of a like nature, at which Newton arrived, "may be yet exhibited in all the fulness of demonstration." I found myself then called upon humbly to entreat Dr Brewster, in this same Journal, to have the kindness to excuse, on that point, the impracticability, under which we in France labour, of admitting such anti-catholic conclusions. The Scotch biographer of Napier recurs with some regret to the expression of repugnance I then gave utterance to, in respect that, according to him, the Commentary of Napier contains, in the passages he cites, more than nine quarto pages of *condensed* proofs of that same proposition. Nevertheless, he has no idea of being scandalized at my blindness. "In the present state of the world, it creates no sensation to hear M. Biot announce, that it is impossible for him to believe the eleventh horn of Daniel to be the Church of Rome; but the times were very different when Napier wrote. To this we must add, that when such Protestants as Calvin and Joseph Scaliger openly avowed their impressions that the whole Revelation of St John was an inexplicable mystery, of which the very writer was a problem, it is greatly to the honour of Scotland, that, from the bosom of so rude a country, a commentary should have come worthy of the first scholar of the age, and capable, as we shall shew, of instructing even our own enlightened times." (P. 201.) If we may be permitted to appreciate this conclusion of our biographer by the light of human understanding alone, I confess I do not see how it flows from the authorities he cites, which appear to me rather to establish a consequence of an opposite nature. But perhaps the inspired character of his text has descended to the panegyrist, in which case I have not a word more to say.*

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* To us it appears that the whole of Mr Napier's analysis of the theological works of the inventor of logarithms, has been entirely misapprehended by M. Biot, and to a degree beyond what we think could have happened to any

The commentary on the Apocalypse was, on the part of Napier, an edifying work, long meditated, which at first he had undertaken with the design of converting the Papists, as he himself narrates in his preface. But the crisis he chose for the publication, affixes to his first project the character of a less charitable intention; for it took place precisely two days after

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English reader. Mr Napier contrasts the present enlightened period, (when no Protestant of enlarged views or common sense is even startled by declared scepticism, as to the truth of such interpretations of the Apocalypse,) with the dark age in which Napier wrote, when Roman Catholic domination, powerfully aided from abroad, was the engrossing object of political and patriotic resistance in Scotland, when the subject of theology had not yet been treated either learnedly or systematically, and when the field of prophecy was yet unexplored by powerful minds. Dr Brewster, in his life of Sir Isaac Newton, had not once mentioned the name of Napier. The biographer of Napier shews that whatever is valuable in Newton's scriptural commentaries is to be found, even more learnedly treated, in Napier, a century earlier, when circumstances rendered such considerations more rational than in the age of Sir Isaac Newton. Mr Napier, contrary to M. Biot's assumption, equally dissents from Dr Brewster's unqualified vindication of the "Newtonian interpretation of the Scriptures." He merely maintains, that for "Newtonian" we ought to read "Naperian," and give the glory *quantum valeat* to Napier. His biographer, moreover, contrasts Napier's very original commentaries, which may be said to have founded the school of *theological learning* in Scotland (a circumstance independent of fanciful interpretations, and therefore biographically valuable), with certain weak, but not unpopular writers of the present day (he instances Cunningham and Keith), whose mystical conceits are unredeemed by solid learning, and not excused by local circumstances. Napier's theological works (strangely overlooked by the learned M'Crie, in his history of that department of letters of which they were the earliest and most conspicuous productions), are an interesting and important step in the progress of Scottish learning, not in respect of their peculiar interpretations, but for the spirit of theological investigation, the learning, and the philosophical method of inquiry therein displayed to a barbarous age and country. The same subject, as handled by Sir Isaac Newton, to whom has been given all the praise, is rather a retrograde step not pleasant to contemplate in the history of letters; and the more recent elucidations that same subject has received from modern enthusiasts, who are much more jealous of their own infallibility than Napier was of his, is simply a page of human folly. This is all we can gather from that portion of Mr Napier's work which M. Biot reviews with such elaborate sarcasm, as if it had been an insidious and illiberal attack upon the Catholics. Nay, so far does M. Biot carry this mistake, as to insinuate, what is equally contradicted by the work itself and by M. Biot's whole abstract of it, namely, that Mr Napier's principal object in this biography was to make the inventor of logarithms' scientific character and history a stalking horse to superstitious Protestant illiberality.—*Translator.*

the importunities of the Presbyterians had extorted from King James the definitive confirmation of the act of excommunication which embraced Napier's own father-in-law. And in the dedication of his commentary, which Napier addresses to James, we may observe with what fanatical violence he discourses: "Therefore, sir, let it be your Majesty's continual study (as called and charged thereunto by God) to reform the universal enormities of your country; and first (taking example of the princely prophet David), to begin at your Majesty's own house, family, and court, and purge the same of all suspicion of Papists and Atheists, or neutrals, whereof this revelation foretelleth that the number shall greatly increase in these latter days. . . . . So also we beseech your Majesty, having consideration of the treasonable practices in these present days, attempted both against God's truth, your authority, and the commonwealth of this country,—to proceed to the other degrees of that reformation, even, orderly, from your Majesty's own person, to your highness's family, and from your family to your court," &c. Napier himself, in his preface, unfolds the motives for that publication. "Yet I purposed not (says he) to have set out the same suddenly, and far less to have written the same also in English, till that of late this new insolency of Papists, arising about the 1588 year of God, and daily increasing within this island, doth so pity our hearts, seeing them put more trust in Jesuits and seminary priests, than in the true Scriptures of God, and in the Pope and King of Spain (it was the time of the Armada), than in the King of kings, that to prevent the same, I was constrained of compassion, leaving the Latin, to haste out in English this present work, almost unripe, that thereby the simple of this island may be instructed, the godly confirmed, and the proud and foolish expectations of the wicked beaten down; purposing hereafter, God willing, to publish shortly the other Latin edition hereof, to the public utility of the whole church." Those Scotch writers who, like Dr Brewster and the present biographer, restore these fine things to light in the present day, would seem to be moved by the same compassion towards us that influenced Napier with regard to the Papists of his times. It is to be regretted that they have not at their command temporal circumstances equally efficacious towards the upholding of their doctrines.

Then was the heyday of sorcerers and witches. They were believed in, and they were burnt. Napier, as his biographer confesses, passed with the vulgar as having his own share of familiar conversation with "*le vieux nick;*" and, indeed, he appears to have been very willing to let the belief go that this opinion was not without foundation. Yet in such estimation was he held that he was never persecuted on that account. He appears to have been in reality occupied with mechanical, and even physical science; for when the English had some reason to dread the Popish fleets in 1596, Napier transmitted to the Scotch ambassador at London a list of inventions, after the manner of Archimedes, to annihilate them. These secrets are, of burning mirrors, of pieces of artillery of a new construction, and a method of navigating under water; but all this is merely announced, not given in detail. Unhappily he was not always so disinterested in the application of his science, as appears from the following contract in which he engaged with one of the most wicked characters of that epoch, called Robert Logan of Restalrig; a contract the whole of which is in the hand-writing of Napier himself, and his biographer has been at the pains to furnish a *fac simile.* This Logan of Restalrig had plunged with daring ferocity into the desperate cabal of Francis Stuart, Earl of Bothwell, in 1594, and by virtue of that declaration of war went robbing and hectoring on the high-roads in the neighbourhood of Edinburgh. The legality of these proceedings not having, unfortunately for him, been recognised, he had been cited before the criminal court, and was outlawed for not appearing. But he troubled himself very little about that matter, as he happened to possess, on the wildest shores of the German ocean, an inaccessible retreat in the tower of Fals-castle, celebrated of late years under the name of Wolfs-crag, by Sir Walter Scott, in the *Bride of Lammermoor.* There, Restalrig, not very well knowing what to do with himself, called to mind an old tradition, according to which some treasures had been, once upon a time, buried within his castle; and being cognizant of Napier as a very learned man, somewhat addicted to necromancy, he made proposals to him that he, Napier, should undertake the search, which the other

accepted, as we shall presently see, in all good faith and sincerity, in the terms and clauses of the following contract, which we translate from the original Scotch as literally as we possibly can.*

"A Edimbourg, le jour de juillet, l'an du Seigneur 1594, il est appointé, convenu et agréé entre les personnes soussignées; c'est-a-dire Robert Logan de Restalrig, d'une part; et Jean Napier, tenant le fief de Merchiston, d'autre part; dans la forme, manière et effets suivants; savoir: Pour autant qu'il existe divers anciens rapports, motifs et apparences qu'il y aurait dans la demeure dudit Robert, au lieu dit Fals-Castel, une somme d'argent monnayé et trésor, déposés et cachés secrètement, le tout quoi n'a pu être découvert par personne, le susdit Jean fera tout son possible et exacte diligence pour le chercher et retirer; et, avec toute l'industrie et science qu'il peut mettre en œuvre, il devra tenter, essayer et extraire le somme dont il s'agit; et, *par la grâce de Dieu*, ou bien il trouvera ladite somme, ou il s'assurera qu'il n'a pas été caché là de pareil dépôt, le tout autant que son travail, sa diligence et sa science pourront le faire. Pour quoi ledit Robert donnera, et, selon la teneur du présent écrit, donne et accorde audit Jean le tiers exact de quelque argent ou trésor caché que ledit Jean trouvera, ou qui sera trouvé par son moyen et industrie, dans ladite place de Fals-Castel ou ses alentours. Et cela pour être partagé par juste poids et balance entre eux, sans aucune fraude, opposition, débat et contention quelconque; de telle manière que ledit Robert devra avoir justement les deux parts, et ledit Jean, justement la tierce part du tout, sur leur foi, parole et conscience. Et, pour le sûr retour et sauve conduite dudit Jean, depuis le susdit lieu de Fals-Castel jusqu'à Edimbourg, sans être dépouillé de sa tierce part, comme sans recevoir aucun dommage, dans sa personne ou les effets à lui appartenant, ledit Robert devra faire convoyer sûrement ledit Jean, et l'accompagner sain et sauf dans la manière susdite, jus-

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* It is curious that this contract should not have been destroyed, even by Napier, and also that it should have survived the accidents of time, to be so admirably translated by a member of the Institute of France in the year 1835. As the original is printed in Mr Napier's work, and a *fac simile* also given, we have thought it better to furnish here the additional curiosity of M. Biot's translation.—*Translator*.

qu'à Edimbourg. Auquel lieu ledit Jean se trouvant revenu sans encombre, il devra, en présence dudit Robert, effacer et détruire le présent contrat, pour pleine décharge des deux parties ayant *honnêtement* satisfait et accompli leur engagement l'une envers l'autre. Et il est arrêté qu'aucune autre décharge que la destruction du présent contrat ne sera d'aucune valeur, force ou effet. Et dans le cas où ledit Jean ne trouverait pas de trésor caché, après tous ses efforts et diligence, il s'en rapporterait pour le dédommagement de ses peines et travail à la discrétion dudit Robert. En témoignage du présent et pour marque de toute honnêteté, foi et fidélité à l'observer dans toutes ses conditions, relativement à chacune des deux parties, ils ont l'un et l'autre souscrit le présent de leurs propres mains, à Edimbourg, les jour et an que dessus.

Signé " ROBERT LOGAN DE RESTALRIG.
" JEAN NEPER, sieur de Merchiston."

How could the great theologian of Scotland, the *marvellous Napier*, as his biographer calls him—how could he have the conscience to enter into such a contract—a contract, moreover, almost of pure necromancy—with a bandit and notorious cutthroat—he who had evinced horror so excessive, and indignation so scrupulous, against the temporal depravities of the Papists, and against those eight-and-twenty Popes reputed to be necromancers? Our biographer does not dissemble the difficulty of this question, and he gets out of it the best way he can, by referring the act in question to the savage rudeness of the times, *and the simplicity of our philosopher's character.* (P. 223.) There may be found, in our opinion, an explication nearer the truth, and more serious, in the doctrine admitted at that time in Scotland, among the casuists of the puritanical league, and revived in the present by another sect, who appear to be making rapid strides in England,—and that is, that all means are good in the hands of the Saints, as they are called, or, in other words, that the Saints cannot sin. The Scotch biographer passes dexterously over the moral consequences of that act, and only takes occasion to call our admiring attention to " the undaunted courage of the man who was willing to go alone with the robber to his cave;" after which he adds, " to pronounce the transaction mercenary would be to apply the fallacious test of modern notions to the dimly seen manners of antiquity." Papists, then, are not the only persons who hold accommodating opinions!

Here terminates all that we have to say of Napier in a political, moral, and theological point of view. We have explained above the considerations which have led us to study him, in the first instance, under that aspect, following the numerous materials presented to us by his recent biographer. It remains for us now to contemplate him as a mathematician, and, thank God, our task will be henceforth much easier. For to restore him as such, the only view of him, in our conception, that merits the regard of posterity, we have only to abstract, so to speak, his own original works, completed, as they are, by several new and curious documents that have been added by his present historian, in which respect it may be said, with justice, that this biography will prove to be of great utility.

SECOND ARTICLE.—Hitherto, we have only discovered in Napier a Scottish Baron of the sixteenth century. Confined to the heart of a savage country, immured within a fortress tower, he lived isolated with his family, without any interchange of thought beyond what the management of his domains, or his unavoidable participation in the political and religious quarrels of his times, exacted. A Presbyterian, rigid and enthusiastic, he commented on the Sacred Writings, after the manner of his day; and under the influence of the same prejudices which inflamed other fanatics of his sect, he expounded, with a reliance not less wrapt, not less darkling, the pretended allusions of the Divine word to those circumstances in which the reformed church then found herself placed.

Well! from the very bosom of such darkness there was destined to spring forth an invention—for I may not call it a discovery—an invention almost mechanical and material, which was to create a revolution in all the methods of arithmetical calculation hitherto employed in the sciences, to bestow upon them a facility, a simplicity, an accuracy, beyond all expectation; even to the extent of suddenly stultifying and annihilating a multitude of numerical tables, previously calculated with inconceivable labour and pains to facilitate mathematical results—toil, to which not only the laborious were and might still have been doomed, but even such geniuses as Copernicus and Kepler; and this, too, without relieving, by that immense and irreparable sacrifice of time, their successors from toil, the very same, or more painful still, in proportion to the advance of applicate science. To liberate the mathematical sciences from chains so grovelling—to liberate them for ever—thus to sweep from the path of genius every obstacle to the immediate realization of all her conceptions dependent upon numbers—behold the triumph of the Logarithms! and that species of emancipation has already had, and will for ever have, an influence over the progress of human knowledge too great not to incite us to the attempt of here rendering intelligible, to every attentive mind, how it was that Napier realized an invention so marvellous.

The Scotch biographer has felt with us the necessity of fulfilling this task. Unfortunately, however, his zeal for his ancestor and his countryman could derive little aid in this attempt, from the writings of mathematicians, even of those whose special design was the history of mathematics. For by a fatality, almost inseparable from those inventors whose fortune it has been to awaken the very train of improvement by which their own discoveries are subsequently brought to perfection, no one, now-a-days, reads Napier's original work, entitled *Mirifici Logarithmorum Canonis Descriptio*, published in 1614, wherein he expounds the mode of generation he assigns to those new quantities named by him *artificial numbers* or *logarithms;* to which he adds their affections or numerical properties involved in the definition; their use in the simplification of arithmetical calculations, when it is necessary to multiply numbers together, or to divide one by the other; their application in the resolutions of trigonometry and astronomy; and, lastly, the numerical tables, containing the logarithms of the trigonometrical lines, termed *sinus*, *cosinus*, *tangentes*, *secantes*, calculated from minute to minute through the whole degrees of the quadrant, which must have cost him an inconceivable physical labour, independently of the invention. All this is given without explication, without any opening as to the train of ideas which had led him to conceive the admirable utility of these tables, no more than he gives us with regard to the means he had employed for the calculations.

Neither do we ever read now his other book, entitled, ***Mirifici Logarithmorum Canonis constructio***, which was only published after his death, by his son, in 1619; a work in which Napier unfolds, establishes, demonstrates, all the processes, all the mechanism of the construction of those logarithmic tables which he did not choose in the first instance to unveil. We are now independent of his method, and of every thing except his primordial idea. The immense development given to the algebraic calculus by the use of literal symbols, the introduction of which is due to Vieta, furnishes us, in the present day, with rapidly and indefinitely converging series, by means of which we obtain those same logarithms by a direct and immediate path, almost without labour, and with a perfect adaptation of symbols, which always enables us to see the present effect of those general operations which we express by formulæ, and permits us to appreciate, with a generalization not less perfect, the degree of approximation of our results. Nevertheless, though the precision be boundless with which these series may be pushed in search of logarithms, I declare, to the honour of Napier, they effect nothing which may not very easily be attained by his original method; and if, as is natural to suppose, this assertion appear somewhat rash to our analysts, I hope immediately to prove it in a manner that will remove every objection.

But, in order to form a just idea of Napier's labours, we must study his own books (especially the second one, wherein he unfolds his method), and not merely rely upon the abstracts that have been given of them. Of all these abstracts the best, that is to say the most concise and elaborate, is, in my opinion, that which Hutton published to his Introduction to the Mathematical Tables of Sherwin, and which is reprinted, with that introduction, in the first volume of *Scriptores Logarithmici.* Yet, after all, this abstract is rather an exact reproduction of Napier's steps, than any attempt to characterize them in their principle, or appreciate them in their results, by a comparison with our actual methods; now, of all others, that is the very point of view under which it is most delightful to contemplate an original inventor. As for Montucla, the popular historian of mathematics, one would almost be tempted to believe that he never had in his hand Napier's posthumous explanatory work, for he attributes to him processes of bisection which are not his, and were subsequently employed by Briggs. We might expect to find a juster estimate in the history of astronomy by Delambre, who was neither deficient in his knowledge of the existing methods of logarithms, nor in his love of truth. But by a defect in philosophy too characteristic of his work, he does not merely make use of the simplicity of our modern formula to illustrate Napier's ideas, which would be their legitimate use; he translates, imperfectly, those ideas into modern formulæ, thus giving them for their base an empirical approximation, which does not belong to them, and which is positively opposed to the genius of Napier. Thus disfigured he submits him to inspection, makes him answerable for inaccuracies which he has not committed, for faults which he attributes to him through blunders of his own, and then pronounces a judgment not the less false, that it is very kind and complimentary.* His recent Scotch biographer relies much upon this authority to enhance the glory of Napier, and exultingly quotes it against some few writers, especially English, who, from sincere scientific opinion, or from national prejudice, have, as he supposes, pretended to depreciate the Scotchman, in attributing the first idea of the discovery of logarithms to an obscure mathematician of the Continent, named Juste Byrge, of whom, indeed, Kepler utters a single word in the introduction to the Rudolphine Tables, as if he had, upon some occasion, imagined something of the kind that he had never published.† But of what use is it to discuss unknown pretensions not brought forward, and which no one, in the present day, can either discover or appreciate. At the epoch of Napier's conception of the logarithms, all mathematicians, all astronomers, who by that time were in great numbers, felt every moment the necessity of some invention which would simplify the frightful numerical calculations to which they were incessantly constrained to devote themselves for the solution of astronomical triangles, the sole application of mathematics at that time known. Various passages of the scientific history of the period bear witness to the attempts made with this object in view, as well by Byrge as doubtless by many others, among whom Kepler reckons himself. And, truly, when we consider what it must have been to calculate numerically tables of sines and natural tangents, for a radius expressed by a million, or even ten millions of parts, of which it was then composed,—when we reflect that all this demanded continual divisions and multiplications which required

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* Delambre's History of Astronomy is one of the greatest works of modern continental philosophy, as its author is one of the greatest names. The analysis of logarithms in that work, however liable to the objections here somewhat harshly pointed out by M. Biot, is a work that would, of itself, have stamped the author as a great mathematical writer; and we cannot see that Mr Napier's reference to it was altogether so rash as M. Biot's observations would imply.—*Translator.*

† Here is the passage in Kepler; he is speaking of the geometrical sexagesimal progressions employed by the ancient astronomers, and of which the successive terms are designed by the characters of degrees, minutes, seconds, thirds, &c.; then he adds, "fui etiam apices logistici Justo Byrgio multis annis ante editionem Neperianam viam prœiverunt ad ipsos logarithmos. Etsi homo cunctator et secretorum suorum custos fœtum in partu destituit, non ad usus publicos educavit." (*Tab. Rud.* cap. iii. p. 11, in fol.) It may be presumed, upon the evidence of this passage, that Juste Byrge had, indeed,

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applied to the ranges of sexagesimal derivations then in use, the observations of Archimedes upon the geometrical and arithmetical progressions, considered in relation to each other; he may perhaps have even perceived the simplifications that might be deduced when it was necessary to multiply or divide such progressions by each other. But, in order to bring within such a progression all possible numbers, a mighty step was to be made, and therein consists the idea peculiar to Napier. Moreover, if Byrge had obtained a glimpse of that idea, he had neither followed it out nor published it; who, then, can assign a value to it in the present day? Lastly, the proof that Kepler did not attach to this remark of Byrge any right, more or less approximating to the invention of logarithms, is, that in those same *Rudolphine's Tables*, he says expressly that Napier is the inventor.—*M. Biot.*

All that is urged in this note by M. Biot will be found to be comprehended in Mr Napier's investigations of the matter. (*Memoirs*, c. X.) Nor can we agree with M. Biot in his somewhat impatient remark, "Mais qu'est-il besoin de discuter des titres inconnus, que l'on ne produit pas, et que personne aujourd'huis ne peut voir, ni apprecier?" He appears to have overlooked Mr Napier's exposition of the very great injustice done to Napier, in this matter, by Dr Charles Hutton; and Montucla and others had attached too much importance to the claim made for Byrge. It was the duty of Napier's biographer to clear up completely, as he has done, and, as we believe, required to be done, the sole and unquestionable right of Napier to the invention of logarithms, both in their original form, which may be termed the *parent* logarithms, and in their modified form, called *common* logarithms, and sometimes attributed solely to Briggs. Mr Napier's investigation of the matter has left no dubiety, and has, moreover, elicited this additional and highly valuabl [illegible]ony and tribute from M. Biot.—*Translator.*

to be unsparingly worked out to the close, without being excused a single figure in the largest numbers,—we may very well understand how the universal desire of mathematicians was bent upon their delivery from this grievous burden, and that the necessity of the case should suggest a thousand means, more or less perfect, for removing it. But, for that purpose, Napier alone has given us—has published the logarithms; no one, before his time or since his day, has hit upon any invention equal to this for the purpose; and it alone serves us still, without permitting us to feel a desire or necessity for any other. By that charter, his right of inventor, of sole inventor, is incontestable. But that right becomes, if possible, yet clearer, when we study the principle of his tables, when we analyze the idea upon which they are based, when we make ourselves master of its originality, and can appreciate the accuracy with which he applies it, and the precision of the results he deduces. This is what I mean to endeavour to make all readers sensible of, reserving the details of the calculus for a note to be placed at the conclusion of this article. If it be my fortune to draw this merit from the tomb to which the commentators had consigned it, I will not pretend to say of him, as Cicero once said of Archimedes, *Humilem homunculum è radio et pulvere excitabo;* but I shall feel confident of having communicated a subject of genuine satisfaction to those enlightened philosophers who love the glory of their predecessors as their own inheritance, and find their own happiness in being able to render a just homage to their labours.

It was that mighty genius of Syracuse who, in his treatise *Arenarius*, was the first to record those properties of numerical progressions upon which the theory of logarithms is founded. Archimedes proposed to himself, not the idle question how many grains of sand might be contained in a sphere equal in diameter to a sphere of the known universe, but to demonstrate that a number as great as that, or infinitely greater, could be specified, and written, by means solely of the numerical characters in use among the Greeks of his time. It is well known that those characters were the letters of the alphabet, which they employed consecutively, in their natural order, simple and accented, to designate the various classes of units, tens, hundreds, thousands, up to tens of thousands of units, which they termed *myriads*, and indicated by the capital letter M, surmounted by that letter of the alphabet which expressed the number of myriads intended. This being arranged, Archimedes supposes an indefinite progression of numbers, commencing by simple unity, successively multiplied by ten, so that when written out, in the notation now in use, the progression would be,

1; 10; 100; 1000; 10000; 100000; &c.

But as in this method of notation we would soon find ourselves embarrassed by the multiplicity of zeros following the unit, we shall abridge the expression by the aid of that ingenious artifice contrived by Descartes, and which consists in only writing out the common factor 10, qualified by a numerical index of more or less value, which marks (becomes the *exponent)* how often the common root 10 is, in the particular term, to be found multiplied by itself. Then writing out, according to this method, the successive terms of the progression, and noting beneath each term the rank which it occupies after the first term, we shall obtain the following lines:—

$1;\ 10^1;\ 10^2;\ 10^3;\ 10^4;\ 10^5;\ 10^6;\ 10^7;\ 10^8;$ &c. indefinitely.
0 1 2 3 4 5 6 7 8

It is evident, upon mere inspection, that the number in the lower line, which expresses the rank of each term, is equal to the exponent which marks how often the common root 10 is *factor* in that particular term. This was not evident to the eye, at a glance, in the literal notation employed by Archimedes; and, moreover, it was not possible for him to express, as we do, the character of indefinite extension which he proposed to give to such a progression. What then was his expedient? In the first place, he considers apart the nine first terms from 1 to 10^8; but he can write out, and even name these terms; for the highest of them, that is to say ten thousand times ten thousand, is only equal to a myriad of myriads. Placing, then, these eight first terms by themselves, he calls them *numbers of the first order*. In the next place, with the ninth term 10^8, he composes a new unity, which he calls of *the second order*, and he arranges these new units like the former, in numbers progressively multiplied by ten, till he arrives at the eighth term of this new order, which is 10^{15}; so that the next term 10^{16}, is in like manner found to be a myriad of myriads of numbers of the second order; and thus, by continuing to compose successive orders of units, the first term of which is always the myriad of myriads in the preceding order, it is evident that he could extend the series as far as he chose, and even express every term orally; for, in order to do so, no more was necessary than to conceive them all placed consecutively after each other, and then to separate them into orders, or octades, as in the following lines:—

$$\overbrace{1;\ 10^1;\ 10^2;\ 10^3;\ 10^4;\ 10^5;\ 10^6;\ 10^7.}^{\textit{1st Order.}}$$

$$\overbrace{10^8;\ 10^9;\ 10^{10};\ 10^{11};\ 10^{12};\ 10^{13};\ 10^{14};\ 10^{15}.}^{\textit{2d Order.}}$$

$$\overbrace{10^{16};\ 10^{17};\ 10^{18};\ 10^{19};\ 10^{20};\ 10^{21};\ 10^{22};\ 10^{23}.}^{\textit{3d Order.}}$$

$$\overbrace{10^{24};\ 10^{25};\ 10^{26};\ 10^{27};\ 10^{28};\ 10^{29};\ 10^{30};\ 10^{31}.}^{\textit{4th Order.}}$$

Thus any term, at whatever distance from the first, may be perfectly defined, and named, by announcing the order or octade to which it belongs, in addition to its place in that octade itself; and, moreover, that mode of characterising will be infinitely more simple than if one attempted to write it out in an explicit manner; for, to take the example, in commencing with the dimensions of a little grain of sand, and rising, from multiplication to multiplication, by means of this series, so as to conceive a sphere, composed of these grains, equal to the sphere of the whole galaxy, Archimedes proves that the sum-total of these little grains will be less than a thousand myriads of numbers of the eighth order; now, from the table given above, it is easy to see that the unit's term of that eighth order will be expressed by the number 10 multiplied by itself 56 times; and as a thousand myriads of units make a thousand times ten thousand, or, 10 seven times factor, we find that the number announced by Archimedes is equal to 10 multiplied by itself 63 times—a number which, even with our Arabic notation, is a long one to write out, being unity followed by 63 zeros. But this matter becomes very simple, and much more so for us than for Archimedes, if we employ the Cartesian method of exponents, which simply expresses how many times the multiplication of 10 by itself has entered into the operation, for then that immense number of Archimedes is written under this little contracted form, 10^{63}.

In all this the simplification results from the fact that, instead of considering the numbers themselves with the multiplicity of characters which express them, we merely designate them by their rank in the indefinite progression, and to express that rank is always by much the shorter process. In following out this idea, Archimedes proves that it is equally available for obtaining, by a very easy process, the products of terms in the progression multiplied together. For example, suppose we wish to multiply the fourth term, which is 1,000, or 10^3, by the fifth, which is 10,000, or 10^4, the product will be 10,000,000, or 10^7; but instead of thus seeking it, and painfully writing out the component characters, it will suffice to add together the figures 3 and 4, which express (are *exponents* of) the rank in the progression, of the two terms which are to be multiplied together. For their sum, 7, marks the number of times 10 is factor in the product sought for, and thus enables us at once to write out the product 10^7. Thus, multiplication is superseded by addition a much more simple operation. Inversely, if this term in the series, 10,000,000, or 10^7, be given, and we are required to divide it by the other term 1,000, or 10^3, we need only take the difference of the exponents, which is 7 *minus* 3, that is to say, 4; and 10^4, or 10,000, will be the quotient sought for, the same which would have been obtained tediously by division. All the other terms of the indefinite series offer the same facility of abbreviation when required to be multiplied, or divided, together; which results from this, that they are derived successively the one from the other by an unvarying ratio, thus forming what we call a *geometrical progression*, or by quotients; while, on the other hand, the more simple numbers which express the rank of each term, increasing simply by one unit, and always one unit in passing from one term to the next, constitutes another kind of progression called, *progression by equidifference*, or *arithmetical progression*. Archimedes detected and demonstrated all the relations, of which we have given the exposition above, betwixt these two kinds of progressions, when their terms are thus brought into correspondence. And in order to shew that these properties were applicable to any terms of the two series, he conceived the idea of representing generally these terms by means of letters employed solely as signs of quantities, without any particular numerical value; thus affording the first example of reasoning applied to figurative symbols representing abstractions, in which, properly, algebra consists,—that powerful intellectual instrument for the discovery of the general relations of magnitudes.*

From this to the logarithms there is but one step, and the logarithms themselves are just indices used, according to the Archimedean principle, to express the rank of each number in an indefinite geometrical progression comprehending all numbers; so that the progress of multiplying and dividing those numbers together may be superseded by the mutual addition and subtraction of the corresponding indices. But how is every number to be comprehended in the same geometrical series continually progressing by equal ratios? It is precisely in this question that Napier's fundamental idea consists. It was only necessary to make that common ratio of increase so little removed from equality that the progression would march by steps excessively slow; whereby any given number, if it did not fall exactly upon one of the terms of the progression, would at least be found comprehended betwixt two terms so slightly differing from each other that the error might go for nothing; or, still better, as Napier did, it was only necessary to conceive the idea of the corresponding geometrical and arithmetical progressions being engendered by the continuous motion of two moveable points, starting together at the same time, but the one marching by a geometrical acceleration, the other with a movement always equi-different and uniform. The simultaneous position of these two moveable points at any given instant of the progressions, will give, in the geometrical progression, the number, in the arithmetical, the corresponding index or logarithm.

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* Mr Napier particularly observes this fact, and also shews that Archimedes, in his very curious work *Arenarius*, obtained a glimpse of those three great features of the modern science of numbers, Arabic notation, the logarithms, and the language of algebra, unconscious, however, of the mighty mysteries he had touched, and which remained to be successively unfolded in after ages. (pp. 343, 344, 348.) It is complimentary to Mr Napier's treatment of his subject, that M. Biot, in this paper, has followed the same plan of explaining and illustrating the logarithms, by giving a history of the first appearance of the logarithmic principle in the *Arenarius* of Archimedes, and then drawing (as Doctor Hutton, and others, had failed to do,) the essential distinction betwixt that glimpse of the principle, and the great invention of logarithms. And it is but justice to Mr Napier's work to observe, that it follows out that historical and numerical exposition, much more fully than M. Biot gives it. Mr Napier also makes this observation, "That the Arabic system itself is essentially logarithmic, and that the properties of the Archimedian theorem may present themselves to a very ordinary calculator upon a consideration of the simple notation 1000." He illustrates this by shewing, that the *cyphers* serve to *number the steps* which the *figure* has taken in the decuple ascending scale of progression; and consequently, that in this view they are actually *indices* of the value of the particular term. So "that the mere *addition* of the *cyphers* in the Arabic scale will afford the same result as the *multiplication* of the terms," &c. (p. 437.) M. Biot's immediate adoption, in his exposition of the logarithmic principle, of the Cartesian *exponential* artifice, which abbreviates the written expression, tends to obscure this fact, which is certainly interesting in a history of the gradual development of that mighty power of numbers, the logarithms.—(*See the Memoirs, p. 435, et infra.*)—*Translator.*

But this simple idea presented, in the attempt to realize it, a great practical difficulty. In order to form the successive terms of the geometrical progression, it is necessary to multiply them successively by their common ratio, as often as there are units in the index of the terms; and here we are again plunged into calculations by multiplication, precisely what we wished to escape from. Napier extricated himself from this embarrassment by an expedient very simple, and replete with ingenuity. He formed his geometrical progression in the descending scale, from large numbers to less, instead of mounting from small numbers to great as Archimedes did; and he took for the constant ratio of the successive terms, that of 10 to 9, or of 100 to 99, or of 1000 to 999, or generally that of a whole power of 10 to the same power diminished by unity. Thus each term could be derived from the preceding one by simple substraction; for, if the first term be, for example, 10000000, and the second 9999999, this last is obtained by cutting off a unit from the former, that is to say, its millionth part. The third term is derived from the second in like manner, by deducting from the second the ten-millionth part of its value, or 0.9999999, according to our decimal notation; and, progressing in this manner, we obtain by simple subtraction as many terms as we wish, all following each other in geometrical progression, according to the ratio selected. The correspondence of the terms, and the indices marking their rank, compose the table exhibited below, in which the succession is indicated to the hundredth term after the first, pushing the value of each term to the seventh decimal.

INDEX OF THE RANK OF TERMS of the geometrical progression, starting from the first.	NUMERICAL VALUE of the successive terms of the geometrical progression.
0	10000000.0000000
	1.0000000
1	9999999.0000000
	0.9999999
2	9999998.0000001
	0.9999998
3	9999997.0000003
	0.9999997
4	9999996.0000006
And so on to the 100th term, which will be	
100	9999900.0004950

Here is precisely the first table formed by Napier; I have only found it necessary to copy it out in order to give an exact idea of his method. We may apply to the terms which compose it all the properties demonstrated by Archimedes in his geometrical progressions, and obtain the same simplifications of the multiplying and dividing them together. But slow as is the ratio of the progression here employed, it is still but the expression of an intermitting change, while the definition of a logarithm requires that we determine the indices of the rank, which correspond to the same terms engendered by a continuous movement. Napier did not obtain the absolute expression of that rectification, as in the present day we can do by means of our differential methods, which enable us to pass without error from discontinuity to continuity. But in comparing the essential conditions of the continuous movement with those of the intermitting change, he establishes assignable limits between which the logarithm of a given number is always comprised; so that if the difference betwixt these two limits is only beyond the order of decimals which we care to keep, either of these limits, or still better, their medium may be legitimately taken for an expression sufficiently approximating to the logarithm sought. Applying this principle to the table, he shews that the logarithm of the first term 9999999 is necessarily comprised betwixt 1.0000000 and 1.0000001, wherefore he takes it as equal to 1.0000005; now the precise value of that logarithm, calculated by the methods now in use, is 1.00000 00500 00003 333, so that Napier's valuation of it is only in error by the third of a unit on the fourteenth decimal of that logarithm. This, then, is the first term of his arithmetical progression corresponding to the geometrical progression which he adopts; and in multiplying them by the series of numbers, 1, 2, 3, &c., which mark the successive rank of the terms of that geometrical progression, he obtains the indices, that is to say, the logarithms of all those terms. This substantially is his mode of operation, and, with some abbreviations, he leads his table of corresponding progressions from 10000000 to 50000000, so as to obtain a numerical progress decreasing in the ratio of 2 to 1. Then, any number being assigned comprised within these limits, he shews how its logarithm may be directly obtained of the requisite approximation, by a comparison with the two terms of the geometrical progression between which it lies. If the given number is without the limits of the table, he shews how to bring it within, and obtain its logarithm. Thus the general problem of intercalating every number precisely, or by approximation, in the same geometrical progression, is completely solved; and thus for every possible multiplication or division of these numbers with each other, there is obtained the same facilities, the same simplifications, which Archimedes had discovered for the particular geometrical progression which he adopted in the *Arenarius.*

Such is the invention of Napier. He has rendered continuous and universal, throughout the whole system of numbers, those advantages which Archimedes had only obtained intermittingly, and for particular numbers. If it be asked why Archimedes did not make this second step, which now appears to us so little removed from the first, a plausible reason, in our opinion, may be found in the nature of the literal symbols employed in his time to designate numbers. For the signification of these characters being absolute, numbers only differing slightly from each other were often expressed by characters having no apparent mutual relation; or, if their expressions had any elements in common, the ratio of their magnitudes to quantities of a different kind was not manifested by the numerical expression itself; whereas, in our actual method of writing the numbers, those two kinds of evidence exist, and, as it were, appeal to the eye, especially when, generalizing the idea which attaches a value of position to the numerals, we extend it inversely to the subdivisions of unity by means of decimals. We have here, then, another of those examples of the influence of symbols over the extension of our ideas, in which the history of mathematics abounds. And, in reference to this subject, be it remarked, that Napier was the first in Europe who employed that generalization, so simple in the method of noting the decimal subdivisions, which was indispensable to effect those successive subtractions, and confine them betwixt the limits assigned to his error. To convince ourselves that that idea was not so easy a discovery as we are apt to believe, now that use has familiarized it, we have only to look at the complicated and nearly impracticable methods by which Stevin, an experienced and ingenious geometrician, essayed to write the decimals, very shortly before. Pitiscus, indeed, substituted the present notation in the second edition of his trigonometry, 1612; and the *Canon mirificus*, in which Napier employs that notation, was not published until the year 1614, which leaves with Pitiscus the merit so far as priority of publication is concerned. But that Napier, who constantly employed it in his tables, must have conceived it independently of Pitiscus, appears incontestible when we consider the number of years which the calculation of those tables must of necessity have occupied. Their whole construction is founded upon the adoption of that notation, and thus they attest the anterior usage, probably much anterior to Pitiscus, who had not employed it in his former edition in 1599.

The system of logarithms adopted by Napier was the most simple and the most commodious which could then be conceived for the formation of the successive terms of the geometrical progression. The tables which he had constructed already offered, in place of the multiplications and divisions, those immense advantages of simplification which we have explained above. Kepler adopted them, and published a copy of them with his Rudolphine tables, of which, as we have observed, he transformed the plan in order to adapt them to the usage of logarithms. But that invention once found, it was easy to see that the particular logarithmic system selected by Napier was not that which adapted itself the most perfectly to our decimal mode of numeration. Professer Briggs of Oxford, a cotemporary of Napier's, conceived a modification which afforded this advantage, and which is the same we now use. It appears that he received that idea from Napier himself, to converse with whom he made several expeditions into Scotland. At the end of Napier's posthumous work, there is an appendix, in which we find the indication of the method employed by Briggs.† Be this

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* Mr Napier shews (p. 364.), that the subject of his biography was busy with the invention of logarithms, and consequently his use of decimal fractions, at least as early as 1594. *See also pp,* 451, 452, 454. for the history of decimal fractions, as first operated with, and written in the present form of notation, by Napier.—*Translator.*

† M. Biot says, " Briggs, &c. en imagina un autre," &c., but immediately adds, " il parait qu'il reçut cette idée de Napier même," &c. The fact is more fully explained in the memoirs, and appears incontestibly to be this. Briggs, whenever he had studied Napier's invention, saw the practical advantage of a modification of the system to suit, for practical purposes, the decimal scale in use; and he commenced his calculations to effect that advantage. But he journeyed into Scotland (a very interesting fact) to consult the inventor himself upon the subject. There he learnt that Napier had long previously conceived the same principle of modification, and intended to realize it by a method which, when he shewed it to Briggs, the latter instantly acknowledged was superior to his own method of that modification. Therefore Briggs

as it may, Briggs skilfully constructed, on this new plan, excellent tables, the most exact, the most abundant in decimals that have been published even to this day. It is a work to be esteemed, not merely for its patience of labour, but for the skilful ingenuity displayed in numerical approximations. On the strength of this amelioration, however, some have occasionally assigned to Briggs a share in the invention itself. Truly this is to confound two very different merits, *genius* and *labour*. But an ardent reverence for discoveries, however, is not a common faculty, and is too often replaced by another less honourable, and that is, the secret inclination of ordinary minds to lower the exalted.

Independently of the merit of the invention, Napier's tables are a prodigy of laborious patience. When we reflect upon the time and toil it must have cost to calculate all those numbers, we shudder at the chances there were of his being arrested in the progress of realizing his idea, and of its dying with him. It has been said, and Delambre repeats the remark, that the last figures of his numbers are inaccurate; this is a truth, but it would have been a truth of more value to have ascertained whether the inaccuracy resulted from the method, or from some error of calculation in its applications. This I have done, and thereby have detected that there is in fact a slight error of this kind, a very slight error, in the last term of the second progression which he forms preparatory to the calculation of his table. Now all the subsequent steps are deduced from that, which infuses those slight errors that have been remarked. I corrected the error; and then, *using his method*, but abridging the operations by our more rapid processes of development, calculated the logarithm of 5000000, which is the last in Napier's table, and consequently that upon which all the errors accumulate; I found for its value 6931471.808942, whereas by the modern series, it ought to be 6931471.805599; thus the difference

(who tells the story himself), casts away all his own calculations made before he had conversed with the inventor, and *adopted Napier's*. This in every sense gives to Napier the right of having himself deduced the *common* logarithms from the parent system. Dr Charles Hutton's extraordinary view of the point in question, as well as his general exposition, so unjust to Napier, of the original invention of logarithms, will be found very thoroughly refuted in the memoirs, p. 383. *et infra.*—*Translator.*

commences with the tenth figure. I calculated, in like manner, the hyperbolical logarithm of 10, after Napier's corrected numbers; I found for its value 2.30258 50940 346, whereas by the tables in use it is 2.30258 50929 940; the actual difference, then, only falls upon the ninth decimal, and that is two places beyond Callet's tables in present and daily use. If Napier could have commanded the services of a country schoolmaster, to calculate, by his own method of substractions, a geometrical progression slower still than what he adopted, a task which he alludes to as a *desideratum*, the tables of Briggs, calculated to fourteen decimals, would have possessed no superiority over his.*

On the Quadrupeds and Birds inhabiting the County of Sutherland, observed there during an Excursion in the Summer of 1834. By P. J. Selby, Esq. F.R.S.E., F.L.S. &c. &c. (Continued from p. 161.)

AVES.

1. Aquila chrysaeta, *Golden Eagle.*—In the mountainous districts this species is still tolerably abundant, although every device is put in practice to capture or destroy them by the appointed fox-hunters and shepherds, the premiums paid for the adult birds, as well as the eggs and young, being liberal. They attack and often prove very destructive to the young lambs, particularly when their eyry is not far distant from the lambing district of a farm. They are sometimes taken in traps, but more frequently shot, after patient and sometimes long-continued watching. They breed in the highest and most inaccessible precipices, and it is rarely that the young or eggs can be got at, even by the dangerous experiment of suspending a person by a rope from the summit of the cliff in which the eyry is placed. Several hairbreadth escapes, as well as fatal accidents, were narrated to us by individuals who had been engaged in these perilous undertakings.——2. Haliaetus albicillus, *Cinereous Sea-Eagle*: Upon the northern precipitous coast of Sutherland the great sea-eagle is still frequently seen soaring above the waters, or his hoarse bark heard

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* This terminates M. Biot's general abstract of the life of Napier, and the principle of his great invention. We shall take another opportunity of laying before our readers the remainder of M. Biot's paper, which is occupied with a review of Napier's minor inventions, and a scientific analysis of his canon of the logarithms.—*Translator.*

when nearly beyond the ken of sight, though persecuted as assiduously as the former species, being considered as equally destructive to the flocks. It breeds upon the highest maritime cliffs, such as those of Far-out-head, Whiting-head, &c., as well as upon islands in some of the larger fresh-water lochs. At Far-out-head we contemplated for some time the evolutions of two adult birds of this species, almost within gun-shot beneath us, and which were supposed to have their eyry in the face of the rock. Their flight was easy and graceful, and in large winding sweeps.——3. Pandion haliaetus, *Osprey.* The osprey appears to be most abundant upon the western coast of Sutherland, affecting the numerous salt-water inlets or lochs which indent that rugged and rocky barrier of the county, and more particularly those into which the larger streams discharge themselves, as it is upon the *Salmonidæ* that they appear chiefly to subsist. At the mouth of the *Saxford*, a celebrated salmon stream (as the name imports), we observed three or four upon the wing at a time, sometimes soaring at great height in extensive circles, at others hovering over the channel of the river where it enters the sea, intently watching and seeking their slippery prey beneath. They hang suspended in the air like the kestril, but with a slower motion of the wings, and their plunge is made with a rapidity almost incredible, and with an impetus so great as completely to submerge the entire body of the bird. Here I cannot help remarking the beautiful adaptation of the plumage of this bird to its mode of life, for instead of the long lax plumes which adorn the thighs of the terrene raptores, the osprey has these parts covered with close set feathers, and the whole of the under-plumage bears a strong analogy to that of the Natatores. When successful they bore off their prey in their talons to the summit of some of the neighbouring hills, there to devour it at leisure. Their food at this time mostly consisted of the sea-trout, *Salmo trutta*, which had just commenced running, that is, were quitting the sea for the rivers and fresh-water lochs. It not unfrequently happens that they grasp at more than they are able to accomplish, and pounce a fish too large and heavy to raise out of the water. Upon such occasions, after continuing the struggle for some time, they at length relax their hold, which they do without difficulty, though we are told, that, under such circumstances, the bird is generally drowned, being unable to extricate its talons from its finny prey. Mr Baigery, the factor of the Laurie district, to whom we are under many obligations, a short period before our arrival had witnessed an interesting struggle of this kind, in which the bird, after repeated attempts to raise the fish (a large grilse or salmon) was finally obliged to quit his hold, and suffer it to escape. The osprey is also to be seen upon most of the larger fresh-water lochs. At Assynt a pair have long had their eyry upon the remains of an ancient castle about a mile below Inch-na-Damff. They remain generally unmolested by the hunters, as they never attack the lambs, their food being entirely restricted to fish.——4. Falco islandicus, *Jer-Falcon.* I do not insert this noble species as a constant inhabitant of Sutherland, but as an occasional visitant. At Keoldale we met with a recent skin (now in my possession), and the remains of one that had been killed near that place a short time before our arrival by Mr Leslie.——5. F. peregrinus, *Peregrine Falcon*. Is pretty generally distributed, and preys much upon the various aquatic fowl, as well as on grous, ptarmigan, and alpine hares.——6. F. tinnunculus, *Kestril*. Common throughout the county.——7. Buteo vulgaris, *Common Buzzard*. This species we met with in various parts of the county. ——8. Circus cyaneus, *Hen Harrier*. A single female individual was seen and fired at between Alt-na-Harrow and Loch Laighal. No owls were met with, but from the information we received, the Otus Brachyotos inhabits the country.——9. Hirundo rustica, *Chimney Swallow*. Common.——19. H. urbica, *Martlet*. Plentiful. Breeds in great numbers about the marble rocks near Inch-na-Duriff.——11. H. riparia, *Sand Martin*. A few were seen, but no great breeding-station noticed.—— 12. Cypselus murarius, *Common Swift*. We met with this bird in the parish of Durness, where it breeds in the great *Smoo Cave*, and other caverns of the limestone rock.——13. Muscicapa grisola, *Spotted Flycatcher*. A specimen seen at Rosehall, below Oikel Bridge.——14. Merula musica, *Song Thrush*. Plentiful throughout the whole country, particularly where birchen or hazel copse abounds; it is also frequently seen in rocky situations, perched upon a large stone, pouring forth its melodious strains, which are precisely similar to those of its fellow species in other parts of the kingdom. No other thrush nearly resembling the common kind in colour, but darker and smaller, was seen, and we in vain tried to recognise the little brown thrush of Mr Laidlaw, which I am inclined to suppose was only the common species seen in rather unwonted situations. We shot several for examination, in wild and unfrequented places, and where no one, judging of the habits of the thrush, as seen in more southern districts, would have expected to find them. We were informed at Tongue, in which district from the plantations and numerous birch-woods they are very numerous, that they do not migrate, but remain during winter upon the margins of the Firth, and other low situations, where the snow rarely lies, and where they always have a plentiful supply of food. ——15. Merula viscivora, *Missel Thrush*. Was seen about the birchwoods upon the banks of Loch Naver, and in the neighbourhood of Lavig. ——16. M. vulgaris, *Blackbird*. Is not frequently seen in Sutherland. We met with it at Tongue, where it finds an appropriate retreat in the plantations around Tongue House.——17. M. torquata, *Ring Ousel*. This species is very abundant during summer upon all the rocky mountains. It begins to breed immediately on its arrival in April, and we found the young generally flown by the middle of June.——18. Cinclus aquaticus, *European Dipper* or *Water Ousel*. In a district abounding in rocky streams and situations so congenial to the habits of the Dipper, we naturally expected to find this favourite little bird in great profusion. Our surprise at first was therefore great, to meet with only an individual here and there, and those in the most sequestered spots. The cause, however, was soon explained, when we learnt that a decree of extermination had gone forth a few years ago, against this pretty little warbler, and a price set upon its head as the supposed destroyer of the spawn of the salmon; I say the supposed destroyer, as I do not think a case sufficiently strong has been made out against it to warrant so fatal a sentence. That it may occasionally devour the spawn of the salmon and trout, I do not deny; but I hold, that from the depth at which the *impregnated ova* are deposited in the gravel, the Dipper cannot possibly arrive at them or commit any serious injury, and that it is only such ova as have escaped impregnation, and therefore float loose, or such as have not been sufficiently covered, and would therefore perish under any circumstances, that find their way into the stomach of the bird. In Sutherland it goes under the name of the King's-fisher.——19. Saxicola œnanthe, *Wheat Ear.* This clean-looking active bird is very plentiful, and generally distributed over the country, but I think most abundant in the limestone districts, where the superior nature of the soil, and the quality of the rocks, in all probability produce a greater abundance of food.——20. S. rubetra, *Whin-Chat.* A few pairs of this species were seen in various parts, as at Lairg, Tongue, &c., but generally where low copse was met with.——21. S. rubicola, *Stone-Chat.* Was also occasionally met with.——22. Erythaca rubecula, *Redbreast.* Was seen at various stations.——23. Salicaria phragmitis, *Sedge Warbler.* This was one of the few warblers we traced to the northern extremity of the island; it was pretty generally distributed along the margins of the lochs, particularly where low birchen copice and reedy grass abound. The well known babbling notes of this wakeful little bird proclaimed its presence in many unexpected situations.——24. Curruca cinerea, *Common Whitethroat.* Is of rare occurrence in Sutherland. One was seen and repeatedly heard near Tongue House in a young plantation, and we again met with it upon the southern confines of the county, near Bonar Bridge.——25. Sylvia trochilus, *Willow Wren.* The only species of the genus Sylvia we met with, was the common willow-wren, which extended in considerable numbers to the extremity of the island, wherever copse or birch-wood abounded. About Tongue it was very plentiful, and the same at Lairg, the margins of Loch Naver, and the wooded banks of Loch Assynt.——26. Parus cœruleus, *Bluecap Titmouse.* Was seen at Rosehall, in the fir plantations.——27. P. ater, *Cole Titmouse.* Also seen at the same place.——28. Accentor modularis, *Hedge Accentor.* Was met with at all our various stations, and twice seen at a considerable elevation.——29. Motacilla alba, *Pied Wagtail.* Generally dispersed.——30. Motacilla boarula, *Grey Wagtail.* Upon most of the rivers and margins of lochs.——31. Anthus pratensis, *Meadow Pipit.* Very common throughout the county, and met with on the summits of the highest hills.——32. Alauda arvensis, *Sky-Lark.* Very plentiful throughout the country, and was seen the previous year within a few hundred yards of Cape Wrath.——33. Emberiza miliaria, *Common Bunting*. Very common in the lower grounds, particularly where cultivation existed, and was traced to the northern coast of the county.——34. E. citrinella, *Yellow Bunting*. Was seen at all our various stations.——35. E. Schœniculus, *Reed Bunting*. Common upon the margins of all the lochs, and in the swampy districts.——36. Passer domesticus, *House Sparrow*. Was observed in all the villages, and many nest holes apparent in the thatch of Keoldale House, &c.——37. Fringilla cœlebs, *Chaffinch*. Seen about Lairg, Tongue, and Incha na-duriff.——38. Linaria cannabina, *Common Grey Linnet*. Seemingly a rare species in Sutherland. A single pair was seen at Keoldale.——39. L. montium, *Mountain Linnet or Twite*. A plentiful species and very generally distributed. It was first met with at Lairg, and afterwards occurred at all the different stations we occupied. Its song is pleasing, though scarcely equal in compass to that of L. cannabina.——40. L. minor, *Lesser Redpole Linnet*. Was met with wherever birch copse occurred. Several were shot, but all appeared of the common species, and none could be assigned to the larger variety or L. borealis.——41. Sturnus vulgaris, *Common Starling*. Is met with upon the northern and western coasts of Sutherland, where it breeds in the holes and caverns of the limestone rock. We saw several about the Smoo Cave, and a large flock at Scourie.——42. Corvus corax, *Raven*. This powerful bird is still plentiful in Sutherland, although every exertion is used to destroy it, on account of the frequent attacks it makes upon the sick sheep and new dropped lambs.——43. C. cornix, *Hooded Crow*. This is the common crow of the county, the C. corone, or carrion crow, being unknown or a very rare visitant. It is a great destroyer of the eggs, as well as the young, of the various grous, young hares, &c. It generally makes its nest about the root of some birch or mountain-ash, growing out of the face of the rocks or deep ravines, or where beech-woods abound in the highest trees.——44. C. frugilegus, *Rook*. A small rookery exists at Aucheny, about four miles above the bridge of Thin, and we saw a small flock on our return, about three miles above the bridge of Oikel. Towards autumn we were told that great numbers of rooks spread themselves over the county, ascending the mountains to a considerable altitude, where they feed upon the larvæ of *Tipulidæ*, &c., and alpine berries.——45. Troglodytes Europæus, *Common Wren*. Was seen at Tongue and Lairg.——46. C. canorus, *Common Cuckoo*. The cuckoo we found very numerously distributed over the country, and its well known notes were heard for some time after our arrival in every direction. As with us (upon the Northumberland moors) it generally makes use of the pipit's nest, wherein to deposit its egg, and the young, as well as the eggs, are frequently found by the shepherds. The larvæ of the nocturnal Lepidoptera, particularly of the genera *Lasiocampa*, *Odonestis*, and *Saturnia*, are very numerous, and afford it a constant and luxurious repast.——47. Columba palumbus, *Ring Pigeon*. The ring pigeon was observed as far north as Tongue, where the plantations and birch-woods about the base of Ben-Laighal afford it a retreat. A few pairs only were seen during our excursion.——48. C. livia, *Rock Pigeon.* This species, the stock of our common dovecote pigeon, is found in its wild state along the whole of the northern coast of Sutherland, inhabiting the caves and rocky precipices, which rise in parts to several hundred feet in height. It is abundant about Whiting Head and the eastern shore of Loch Eriboll, composed of rocks abounding in caves and deep cleft fissures. The prevailing colour is a dark blue, the wings with two black transverse bands, and the lower back white.——49. Tetrao tetrix, *Black Grous.* Plentiful in all the districts about Lairg and Loch Shin, the base of Ben Laighal, Ben Hope, &c.——50. Lagopus Scoticus, *Red Grous* or *Red Ptarmigan.* In the midland district of the county, between Lairg and Tongue, the common red grous seems abundant. Upon the western coast it is not so numerous, the face of the country being too rocky and sterile, and seldom affording any extended tract of heath.——51. L. mutus, *Common Ptarmigan.* Plentiful upon all the mountains, their rocky summits being favourable to its habits. A specimen was shot by Sir William Jardine, which Dr Richardson, when shewn to him, thought to be the *L. rupestris* of the *Faun. Bor. Amer.* It is smaller than the usual average size of the common ptarmigan, and the plumage is more varied with reddish-brown. It was killed upon the Ben-More ridge above Inch-na-Damff, and we hope to obtain additional specimens from the same locality, so as to enable us to determine the species.——52. Perdix cinerea, *Common Partridge.* A pair was seen at Inch-na-Damff, and about Lairg their call was repeatedly heard.——53. Ardea cinerea, *Common Heron.* Was seen upon the Oikel.——54. Numenius arquatus, *Common Curlew.* Very abundant in all the central parts of the county, where heath and extensive marshy tracts prevail. Upon the rocky western coast it is comparatively rare.——55. N. phæopus, *Whimbrel.* Was seen upon the margin of Loch Shin, but no eggs or young were obtained.——56. Totanus calidris, *Redshank.* Was found breeding on the marshy margin of Loch Doulich, near Lairg, and at the head of Loch Naver. When disturbed from its nest, and as long as the young are unable to fly, the old birds are very vociferous, and wheel around the intruder in circles, making frequent stoops, as if to strike at the head, like the common lapwing.——57. T. hypoleucos, *Common Sandpiper.* Very abundant upon the margins of all the numerous lochs and rivers.——58. T. glottis, *Greenshank.* This species, whose nest had never before been found in Britain, we detected breeding in various parts of the country, generally in some swampy marsh, or by the margin of some of its numerous lochs. It is very wild and wary, except when it has tender young, at which time, when first disturbed, it sometimes approaches pretty near, making a rapid stoop like the redshank at the head of the intruder. If fired at and missed, which is frequently the case even by a good marksman, as the stoop is made with remarkable rapidity, it seldom (at least for that day) ventures again within range. A pair which had their nest in a marsh near Tongue, after having been once fired at, could not again be approached, but we obtained one of the young, apparently about a fortnight old, by means of a water-dog. Another pair were shot near Scourie, by the margin of a small loch, where, from their violent outcries and alarm, they evidently had their nest or young, though we were unable to find either.——59. Scolopax gallinago, *Common Snipe.* Is very abundant in all the moory and marshy tracts.——60. S. gallinula, *Jack Snipe.* The gamekeeper of the Tongue district assured us that the jack snipe breeds in Sutherland almost every year, and that he had obtained the eggs, as well as young, in some boggy ground, about two miles from Tongue. He shewed such an intimate knowledge of the bird, as to do away with any impression upon our minds of a mistake as to the species. Sir William Jardine accompanied him to a spot where he had frequently seen them in summer, but he was not so fortunate as to meet with any. The situation and ground was, however, apparently exactly suited to their habits. In winter it is plentiful in the lower springs.——61. Tringa variabilis, *Dunlin* or *Purre.* In the summer or dunlin plumage we found the common purre abundant upon the margins of all the lochs. The nest is usually placed under the shelter of some tuft or bush, removed a short distance from the usual water-line of the loch.——62. Crex pratensis, *Meadow Crake* or *Corn Crake.* Is very abundant in all the lower Straths, where cultivation exists.——63. Fulica atra, *Common Coot.* Was heard among the reeds which skirt the southern side of Loch Doulich, about two and a half miles from Lairg.——64. Hæmatopus ostralegus, *Oyster-Catcher.* Common upon the margin of Loch Shin, where it breeds, and upon most of the salt-water friths and lochs, such as those of Eriboll, Tongue, &c.——65. Vanellus cristatus, *Common Lapwing.* Very abundant throughout the county.——66. Charadrius pluvialis, *Golden Plover.* Plentiful throughout the county, but particularly abundant in the district between Lairg and Tongue, the parish of Durness, Scourie, &c. Sutherland appears to be the great breeding-station of this species.——67. C. Hiaticula, *Ring Plover* or *Ring Dottrel.* Upon the margins of all the lochs and larger streams very numerous.——68. Anas ferus (*Segetum*, auct.) *Bean-goose.* We were agreeably surprised to find that the bean-goose annually breeds upon several of the Sutherland lakes. The first intimation we received of this interesting fact was at Lairg, where we were informed that a few pairs bred upon some islands about twelve miles up Loch Shin. We accordingly took boat the following morning, and upon arriving at the place, discovered a single pair, attended by four or five young goslings. None were obtained, as the old birds, being wild, escaped seemingly uninjured, although repeatedly fired at, and the goslings immediately dived and escaped into the reeds and other herbage. Upon Loch Naver we also found several pairs at- ended by their young, seemingly about a fortnight or three weeks old, ne of which, after a severe chase, we procured. Upon the islands of och Laighal, from thirty to forty pairs, we were informed, annually had heir nests. We saw several old birds, and the nests that had been used, which are concealed in heath upwards of three feet in height, that covers the islands. The eggs were all hatched, and most of the young had betaken themselves to the neighbouring moors, where they continue till able to fly, secreting themselves, when disturbed, in the highest heather. At Tongue we saw some goslings about a month old (following a hen), which had been hatched from eggs taken at Loch Laighal. We were told that they became nearly as tame as common geese, but refuse to intermix or breed with them. The eggs, from five to seven in number, are smaller than those of the common goose, but of a similar shape and colour. ——69. Anas boschas, *Common Wild-Duck* or *Mallard*. Was seen upon most of the lochs and marshy moors.——70. Mareca penelope, *Wigeon*. As the Wigeon had not previously been detected breeding in Britain, we were much pleased to observe several pairs upon the smaller lochs near Lairg, which we concluded had their nests among the reeds and other herbage which grew in their vicinity. We were not so fortunate, however, as to find one here, though diligent search was made, but afterwards upon one of the islands of Loch Laighal we sprung a female, which we shot, from her nest, containing seven eggs. It was placed in the heart of a large rush bush, and was made of decayed rushes and reeds, with a lining of warm down from the bird's body. The eggs were smaller than those of the wild duck, and of a rich cream-white colour.——71. Fuligula marila, *Scaup Pochard*. A single female was shot by Sir William Jardine, in a small loch between Loch Hope and Eriboll; she was attended by a young one, which unfortunately escaped among the reeds. This is the first instance of its breeding in Britain having been ascertained that I am aware of.——72. Mergus Merganser, *Goosander*. Two or three birds of this species were seen during the excursion, but no nest or breeding station detected. ——73. M. serrator, *Red-breasted Merganser*. Is very plentiful upon all the lochs. At the time we were in Sutherland few had commenced incubation.——74. Podiceps minor, *Dobchick* or *Little Grebe*. Was met with occasionally upon the smaller lochs during the excursion.——75. Colymbus glacialis, *Northern Diver*. A single pair was seen in the Bay of Balnikiel, mouth of the Durness Frith, both adult birds, and in perfect summer plumage. It is probable that they had their nest upon one of the numerous islets that abound in the bay.——76. C. arcticus, *Black-throated Diver*. This beautiful species, whose breeding station had never before been detected, we found upon most of the interior Sutherland lochs. The first we noticed was at the foot of Loch Shin, where we were so fortunate as to find the nest, or rather the two eggs, upon the bare ground of a small islet, removed about ten or twelve feet from the water's edge. The female was seen in the act of incubation, sitting horizontally, and not in an upright position, upon the eggs. In plumage she precisely resembled the male, and when fired at immediately swam, or rather dived off to him at a short distance. Our pursuit after them was, however, ineffectual, though persevered in for a long time, as it was impossible to calculate where they were likely to rise after diving. Submersion frequently continued for nearly two minutes at a time, and they generally reappeared at nearly a quarter of a mile's distance from the spot where they had gone down. In no instance have I ever seen them attempt to escape by taking wing. I may observe that a visible track from the water to the eggs was made by the female, whose progress upon land is effected by shuffling along upon her belly, propelled by her legs behind. On the day following (Saturday the 31st of May), Mr J. Wilson was fortunate enough to find two newly hatched young ones in a small creek of Loch Craggie, about two and a half miles from Lairg. After handling and examining them, during which the old birds approached very near to him, he left them in the same spot, knowing that we were anxious to obtain the old birds. Accordingly, on the Monday morning we had the boat conveyed to the loch, and, on our arrival, soon descried the two old birds, attended by their young, and apparently moving to a different part of the loch. Contrary to their usual habit at other times, they did not attempt to dive upon our approach, but kept swimming around their young, which, from their tender age, were unable to make much way in the water, and we got sufficiently near to shoot both of them through the neck and head, the only parts accessible to shot, as they swim with the whole body nearly submerged. The female could only be distinguished from the male by a slight inferiority of size, and both were in the finest adult or summer plumage. We afterwards saw several pairs, upon various lochs, and upon Loch Kay a pair, attended by two young ones, nearly half grown. When swimming, they are in the constant habit of dipping their bill in the water, with a graceful motion of the head and neck.——77. C. septentrionalis, *Red-throated Diver*. Also breeds upon many of the lochs. We obtained no eggs or young, but it was evident from the conduct of the birds, that they were breeding.——78. Uria Troile, *Foolish Guillemot*. Is common upon the northern and western coasts of Sutherland, and breeds in great numbers upon the precipices of an island, about six miles from Scourie.——79. U. Grylle, *Black Guillemot*. Is also frequently seen. Mr J. Jardine noticed it about the caves near the mouth of the Durness Frith.——80. Alca Torda, *Razor Bill*. Also common.——81. Fratercula arctica, *Puffin*. Common upon the coast and salt water inlets.——82. Phalacrocorax Carbo, *Cormorant*. Numerous in all the friths.——83. P. cristatus, *Crested* or *Green Cormorant*. Is equally plentiful, and breeds upon the rocky precipices of Loch Eriboll, and the western coast.——84. Sula Bassana, *Solan Goose*. Many were seen hovering over the sea, off *Far-out-head* and other parts of the northern coast.——85. Sterna Boysii, *Sandwich Tern*. Was seen upon the Friths of Tongue and Eriboll.——86. St. arctica, *Arctic Tern*. Abundant upon all the friths, breeds upon the flat coast of Tongue, &c.——87. Larus ridibundus, *Black-headed Gull*. Plentiful. Breeds among the reeds of lochs Doulich, &c.——88. L. Canus, *Common Gull or Mew*. Has various breeding stations, viz. upon Loch Shin, Loch Laighal, and various smaller lochs.——89. L. Rissa, *Kittiwake*. Common upon the rocky coasts. ——L. argentatus, *Herring Gull*. Was seen upon most of the salt water lochs, but no breeding station observed.——91. L. marinus, *Great Black-backed Gull.* A few were seen upon the friths.——92. L. fuscus, *Lesser Black-backed Gull.* Many colonies of this species were observed, one upon Loch Shin, another upon one of the islands of Loch Laighal, &c.——93. Cataractes Richardsonii, *Richardson's Skua.* Was observed by Sir William Jardine upon the Durness Frith.——94. Phœnicura ruticella, *German Erythæa.* Was twice seen, first at Oikel Bridge, and again at Ron Stall.

A pair of goldfinches (*Carduelis vulgaris*) and a goatsucker (*Caprimulgus europæus*), were seen in a birch wood on the banks of Loch Laighal, by Mr James Wilson, which have not been noticed in the preceding list.

Memoir on the Star-Fish of the genus Comatula, demonstrative of the Pentacrinus europæus being the Young of our Indigenous Species. By JOHN V. THOMPSON, F. L. S., Dep. Inspector-General of Hospitals. Communicated by Sir James M'Grigor, F.R.S. * With a Plate.

IF we were told by any traveller that he had visited an unknown region, where the animals dropt their eggs on trees and shrubs, which there fixed themselves and shot up like parasitic plants on a long stem, gradually evolving, at their extreme end, member after member and function after function, until the young animals became so perfect as to resemble their parents in every essential point,—when their attachment to the connecting footstalk was dissolved, and they became free and locomotive, and betook themselves to the wandering life of the parent stock! few could be got to believe facts so incredible, and so much at variance with the course of nature, as made manifest everywhere and from all time; but if established on incontestable evidence, the highest degree of surprise and admiration would necessarily supplant our incredulity,—voyages would be undertaken, and the curious of every country would flock to witness such an extraordinary anomaly, at the greatest risk and expense. If, then, a fact so contrary to our experience relating to the superior classes of animals should be capable of exciting so great a degree of interest, it may be presumed that an analogous circumstance, now for the first time actually discovered in an animal belonging to one of the inferior classes, must be considered, at least, as highly worthy of the attention of the philosophic naturalist.

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* Read before the Royal Society of London, in June 1835.

It is no uncommon thing, in the inferior classes of the animal kingdom, to find animals permanently attached from the period of their birth, and during the whole time of their existence, familiar examples of which we have in the oyster, anomia, and various other bivalve shell-fish, and in numerous compound animals of the classes Zoophyta and Infusoria. I have also shewn, in my memoirs on the Cirripedes, examples of animals being free and locomotive in their first stages, and afterwards becoming permanently fixed ; but an animal growing for a period as it were a flower, fixed by its stem, and then dropping from its pedicel and becoming, during the remainder of its life, free and locomotive, is not only new, but without any parallel in the whole range of the organized part of the creation. No wonder, then, that any naturalist, on the first discovery of the young animal in its first or fixed stage of existence, should consider it as belonging exclusively to those which are known to be permanently fixed, analogy would permit no other conclusion to be formed, and consequently it could be classed with none other except the crinoideæ, one known genus of which tribe participates with comatula in being locomotive in its advanced stage; so that this circumstance connects all these animals into an inseparable group, with which the present state of our knowledge will not permit us to associate any other of the asteriæ.

When, therefore, I formerly described the young of the comatula* as a new species of pentacrinus, no person could have suspected so anomalous and unexpected a result, as that it was the young state of this curious star-fish, an animal not only free, but leading the most vagrant life of any of the tribe with which it has hitherto been associated by naturalists,—at one time crawling about amongst submarine plants, at others floating to and fro, adhering to thin fragments by means of its dorsal claspers, or even swimming about after the manner of the medusæ. In swimming, the movements of the arms of the comatulæ exactly resemble the alternating stroke given by the medusæ to the liquid element, and has the same effect, causing the animal to raise itself from the bottom, and to advance, back foremost, even more rapidly than the medusa. Fig. 7, Plate II, represents a comatula, after having delivered its stroke to the water.

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* Memoir on Pentacrinus europæus. Cork, 1827.

The evidence of pentacrinus being the young of comatula, rests upon a comparison of the individuals figured 3, 4, and 5, 6, on Plate II, the former being an advanced pentacrinus just beginning to form pinnæ, and the latter the youngest comatula ever taken by dredging. In the pentacrinus, it is to be observed that the arms are just beginning to form pinnæ towards their extremities, that they have the sulphur-yellow colour and dark marginal spotting observable in the other, which shews, in like manner, that the upper pinnæ are first formed; here, Figs. 5, 6, we have about three pair of pinnæ, with two intervening articulations of the arm between each, then three articuli (counting from the apex downwards), and an additional pair of pinnæ just beginning to sprout. From this to the base of the arm are five more articuli, as yet without any pinnæ, the base of each arm on either side presenting one long pinna appropriated to the service of the mouth. On turning the animal over, the dorsal cirri are found to have increased from five to nine, several of them presenting the appearance of recent formation. Individuals a little older are comparatively common, in which the pinnæ are complete, and from this period they appear to form regularly at the apex of the arm, as this goes on extending in length. These small comatulæ still retain the original sulphur-yellow colour towards the apices of the arms, the lower part and body assuming the characteristic red of the adult comatula. From observations repeatedly made, I think it most probable that the comatulæ attain their full growth in one year, so as to be in a condition to propagate their kind the summer following that of their birth. At that time (viz. May and June) these full grown individuals have the membranous expansion inside each of the pinnæ, considerably extended, at least as far as the fifteenth or twentieth pair, these, which are the matrices or conceptacula, at length shew themselves distended with the ova, which in July, and even earlier, make their exit through a round aperture on the fascial side of each conceptaculum, still, however, adhering together in a roundish cluster of about a hundred each, by means of the extension and connection of their umbilical cords. By what means these ova are dispersed, or how they become attached to the stems and branches of corallines, remain to be discovered; but it is strongly to be suspected that the animal is gifted with the power of placing them in appropriate situations, otherwise we should find them indiscriminately on fuci, shells, stones, &c., which does not appear to be the case. However this may be, if we are allowed to assume that the Pentacrinus europæus is the young of comatula, we first perceive the dispersed and attached ova in the form of a flattened oval disk, by which it is permanently fixed to the spot selected, giving exit to an obscurely jointed stem, ending in a club-shaped head, as in Fig. 2, *e*, in which individual the animal is sufficiently advanced to shew the incipient formation of the arms and the mouth with its tentacula, by means of which it obtains the food necessary to its successive growth. At *d* of the same figure is another, somewhat more advanced, in which all the ossicula of the arms are obvious, as far as the bifurcation. At the letters *a*, *b* and *c* are represented, what I considered formerly as completely formed pentacrini, (*a*) from the position shewing the valvular mouth, and (*x*) the anal aperture; (*b*) shews most clearly the cirri or claspers at the top of the stem, and (*c*) that the living principle extends throughout the entire fabric demonstrated by the varied *movements* of the pedicle. At a later period I observed individuals shewing a still higher degree of development, Fig. 4, and in which the arms had the appearance of bifurcating twice towards their extreme ends, and had become of a sulphur-yellow colour, with a zone of dark coloured spots along either margin. Another circumstance confirmatory of these being the young of comatula is derived from these pentacrini being first seen about the time of the dispersion of the ova of the comatulæ, and again entirely disappearing in September, the only season when young comatulæ are to be obtained, and such as are represented Fig. 5 and 6. In these the points of resemblance to advanced pentacrini have been already alluded to, and it is quite evident, that since they became detached, pinnæ must have been added in both directions, both towards the apex and downwards towards the base of the arms. Those specimens which have made a further progress are plentiful, and have all the pinnæ complete down to the bifurcation, with a few additional claspers added at the back. At Fig. 7, a middle-sized Comatula decacnemos is given, as they appear in June when pregnant with ova; and at Fig. 8 is a portion of an arm magnified, with the ova beginning to escape from the conceptacula, which they do successively from the base upwards. Mr Millar, in his laboured but excellent work on the Crinoideæ has figured our comatula in this stage as a new species, under the title of C. fimbriata; indeed, no naturalist who had not investigated their habitudes in their own element, and at all seasons, could possibly arrive at the knowledge of this very remarkable and curious piece of economy, which may be considered as unique. These animals are further distinguished by the peculiarity of having two openings to the intestinal canal, by which they also differ from the rest of the asteriæ.

The great abundance of comatulæ, in the places they inhabit, is not to be wondered at when we are aware how exceedingly prolific they are; thus each arm may be estimated to bear thirty fruitful conceptacles, each producing about a hundred ova, and as there are ten such, this gives 30,000! as the amount of ova produced by a single individual.

Connected with the natural history of the comatula is that of a nondescript parasite, which appears to be a complete zoological puzzle, as it is not possible to determine from its figure and structure to what class it ought to be referred. This little animal is figured at Figs. 9 and 10, much magnified, its natural size not exceeding that of the breadth of the ossicula of the arms of the comatula; it resembles a flat scale, runs about with considerable vivacity on the arms of the animal, and occasionally protrudes a flexible tabular proboscis, ending in a papillary margin. The disk or body is surrounded by eighteen or twenty retractile and moveable tentacula, and beneath is furnished with five pair of short members, each ending in a hooked claw. Query, Is it a perfect animal or a larva, and does it belong to the Crustacea, Annelides, or what?

Explanation of the Figures in Plate II.

Fig. 1. Pentacrinus europæus, a group of the natural size.

Fig. 2. The same magnified; *f*, the basis; *e* and *d*, two individuals in early stages of growth; *a*, *b*, *c*, fully developed individuals. At x letter *a* the vent is seen, and below it the valvular mouth of the animal.

Figs. 3 and 4. An individual of the natural size, and magnified, still more developed, beginning to form pinnæ towards the ends of the arms, as at *a*, *b*; in this the cirri or claspers at the back of the animal are very distinctly seen.

Figs. 5. and 6. A very young comatula of the natural size, and magnified, *a*, *x*, the vent, in front is the star-like mouth. *b*, *b*, Two of the dorsal cirri.

Fig. 7. Adult Comatula decacnemos. *c*, The dorsal cirri.

Fig. 8. Part of one of the arms seen in face. *a*, Ova protruding from the conceptacula. *b*, Ova just beginning to make their exit. *c*, One as yet filled with the ova. *d*, Ova magnified.

Figs. 9 and 10. Parasite of comatula magnified. 10. Turned over on its back; *m*, Mouth; *f*, feet; *b*, ova?

On the Chemical Constitution of Gadolinite. By A. Connell, Esq. F.R.S.E., &c. Communicated by the Author.

According to the analysis of this mineral by Berzelius, its constituents are yttria, protoxide of cerium, protoxide of iron, and silica. It would appear, however, that Ekeberg had found about 4 per cent. of glucina in a variety analyzed by him; and very lately an analysis has been published by Drs Thomson and Steel, of a variety in which so large a proportion as 11.60 per cent. of glucina appears.[*] The external characters of this latter variety agree sufficiently with those usually assigned to gadolinite; but the proportion of oxide of cerium is considerably less than in the analysis of Berzelius, as appears from the following comparison between one of Berzelius' analysis and that of Dr Thomson:—

	Berz.	Thom.
Yttria,	45.	45.
Glucina,		11.60
Protoxide of Cerium,	17.92	4.33
Protoxide of Iron,	11.43	13.59
Silica,	25.8	24.33

Farther, Dr Thomson's specimen was a large mass of the mineral, weighing several ounces, and mixed with grains of platinum, whilst, so far as I am aware, gadolinite had previously been only observed crystallized, or disseminated in small grains or globules, in other minerals. Its locality, also, was unknown.

Although no doubt can be entertained that the mineral analyzed by Dr Thomson was a variety of gadolinite, yet the peculiarities above alluded to appeared to me to make it a matter of interest to examine again some specimen of the ordinary varieties of this mineral, particularly with a view to the question whether glucina enters into their constitution in notable quantity.

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[*] Records of Science. June, 1835.

The only specimen of gadolinite which I could procure, capable of affording a quantity with which I could attempt determining the proportion of its constituents, (and that much less than I could have wished, being only ten grains,) was one of which the locality was not more particularly marked than as being Fahlun; and which exactly resembled, both as to the matrix and as to the mineral itself, specimens which I have seen labelled as from Broddbo near Fahlun. The gadolinite which it contained occurred in grains of the size of a small pea disseminated in granite. Its colour was black, or very dark green; lustre vitreous, and fracture conchoidal; and it possessed all the other usual external characters of the mineral.

Besides this specimen, which was the principal subject of analysis, two others, of smaller size, were examined generally, the one from Finbo, and the other, I have reason to believe, from Broddbo.

The manner of proceeding was as follows:—The mineral, reduced to fine powder, was boiled in nitro-muriatic acid; and the silica afterwards obtained in the usual manner. From the solution of the other constituents, the oxide of cerium was separated by means of crystals of sulphate of potash, and subsequent solution of the double salt, and precipitation by boiling with potash. Thus far the analysis is sufficiently simple; but the best method of obtaining the remaining constituents is not so easily determined. To separate the oxide of iron, I tried benzoate of ammonia, in the preliminary examination of one of the smaller specimens, but I found, as Dr Thomson also observed, that not merely benzoate of iron was thrown down, but some of the other constituents of the mineral also. I then had recourse to the method of tartaric acid, and hydrosulphuret of ammonia. The liquid, which had been acted on by the crystals of sulphate of potash, was precipitated by ammonia, and the precipitate, after being collected on a filter, was dissolved in muriatic acid. Solution of tartaric acid, and afterwards ammonia in excess, were added, and the iron precipitated by hydrosulphuret of ammonia. The residual liquid was then evaporated to dryness, and the residue completely incinerated by long exposure to a red heat. This ignited matter, of course, was the yttria in a state of purity, or combined with glucina, if the mineral contained that earth. The most obvious method of investigating this latter point was to redissolve the ignited matter in muriatic acid, and treat the solution with caustic potash; and in this way I was enabled to detect glucina in the mineral. I found, however, that, from the much greater relative quantity of yttria, this process was extremely imperfect; and the method, therefore, which I adopted was, in the first place, to throw down as much of the yttria as possible from the muriatic solution, by means of oxalic acid, after nearly neutralizing it by ammonia. The oxalate of yttria was then separated by filtration; the remaining liquid precipitated by ammonia; and the precipitate boiled with caustic potash, and then filtered from the undissolved matter. From the alkaline solution a gelatinous substance was obtained by supersaturation with muriatic acid, and precipitation by ammonia, which was found to be soluble in carbonate of ammonia, and to give with nitrate of cobalt, before the blowpipe, a black or dark grey glass; and, in short, to have all the properties of hydrate of glucina. The matter left undissolved by the alkaline ley, was once more dissolved in acid, and treated with caustic potash, to complete the separation of the glucina from the small quantity of yttria which had not been thrown down by the oxalic acid.

In the principal analysis, with the view of diminishing the great tediousness of the incineration of the yttria and glucina, oxalic acid was employed to precipitate as much yttria as possible, before separating the iron; and the iron was then obtained as before, by precipitating by ammonia, redissolving the precipitate, and then employing the method of tartaric acid, and hydrosulphuret of ammonia. The glucina and remaining yttria were then separated as formerly. But although in this way there is much less matter to incinerate, the other method appears to be preferable in other respects.

By adding oxalate of ammonia to the liquid from which the yttria, glucina, and oxide of iron had been originally separated together, a minute quantity of lime was obtained.

By the process of which the above detail offers a general outline, I obtained from the above-mentioned portion of the specimen marked as from Fahlun, the following constituents:—

Yttria,	36.54
Glucina,	5.90
Protoxide of Cerium,	14.31
Protoxide of Iron,	14.41
Silica,	27.10
Lime,	.45
	98.71

From the smaller specimen, the locality of which was uncertain, but which I have reason to believe was Broddbo, I also obtained a quantity of glucina, although its proportion was not determined. With respect to that from Finbo, I cannot speak with absolute certainty, on account of the very small quantity of matter examined; but from the examination made of it, I have very little doubt that it also contained this earth.

From these researches, therefore, as well as from those of Ekeberg, Thomson, and Steel, it is sufficiently clear that glucina is at least a frequent constituent of gadolinite. It would appear, however, that the relative proportions of its several constituents are subject to variation; and it would be very desirable that any person who could procure a sufficient quantity of crystallized specimens from different localities, should execute a careful analysis of them.

Description of a New Detached Pendulum Escapement; invented by Alexander Witherspoon, Watchmaker, Tranent.[*]

Considering the great variety of the escapements which have already been devised, it may appear difficult, if not impossible, to propose any other constructed on principles entirely new; yet I flatter myself, that that which I am about to lay before the Society of Arts will be found to possess more simplicity, and to approach nearer to perfection, than any which has yet been described.

To obtain a correct notion of the advantages expected from it, it may be proper to glance at the general principles according to which instruments for measuring time are constructed.

Both in clock and in watch movements, time is measured by the oscillations of a body impelled towards a position of rest by a force which increases with its distance from that central position. In watches, this force, being supplied by the flexure of the balance spring, is accurately proportional to the distance of evagation; but in clocks, being obtained by the circular motion of the pendulum, it is only approximately so. The oscillations of the balance of a watch, however unequal in extent, are thus performed in equal times; while those of the clock pendulum deviate a little from perfect isochronism.

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[*] Read before the Society of Arts, 13th April 1831.

The balance or pendulum is the real time-measurer, the train of wheels being attached only for the purposes of counting the vibrations, and of supplying the slight loss of momentum which attends the motion of every piece of mechanism. For these purposes, it it absolutely necessary that the motion of the train be connected with that of the pendulum; the apparatus for making this connection is called the escapement, and on the construction of this escapement the accuracy of the time-keeper mainly depends.

The earliest contrivances of this nature were called *recoil escapements*, because that, during part of the oscillation, the whole train, and along with it the maintaining force, is driven backwards by the momentum of the vibrating body. During the entire oscillation, the train is in connection with the pendulum, the impulse teeth rubbing upon the backs of the pallets. Now, to obtain accuracy in going, the momentum of the pendulum ought to be gradually generated and extinguished by the sole action of gravity; whereas, with this escapement it is affected by the friction and by the resistance of the maintaining force; the changes, then, to which, from the gradual thickening of the oil, these disturbing forces are liable must occasion considerable errors in the movement.

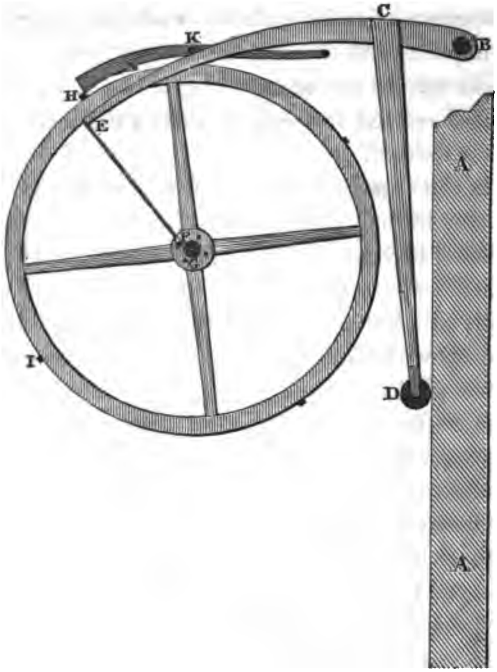
The first amelioration of the common recoil escapement was made by forming the rubbing parts of the pallets cylindric, so that the train might merely be detained by them without being subjected to a recoil. But this still left the motion of the pendulum exposed to the effects of changes in the viscidity of the oil, or in the smoothness of the rubbing surfaces.

The great imperfection of the dead-beat escapement is, that, while the train is communicating no impulse to the pendulum, it continues to retard its motion by pressing upon the back of the pallet, thus creating additional work for itself. This evil has been nearly removed by the contrivance of detached escapements. In these the train is prevented, by means of a detent, from advancing upon or touching any part of the pendulum, which, at the instant when the pendulum requires impulse, is unlocked by it so as to allow the impelling tooth to strike the pendulum and communicate momentum to it; by the time when this communication is finished the detent has returned to its place, and is ready to arrest the next detaining tooth. In this way the train has no communication with the pendulum except while giving it the impulse, and the only disturbing force which exists is the resistance offered to the unlocking of the detent; but this is so feeble, and exerted through so short a distance, that it may almost be overlooked.

Nearly as the detached escapement approaches to perfection, it is still liable to the serious inconvenience of communicating an impulse which, because of the thickening of the oil, gradually weakens, and leads to a diminution of the arc of vibration, and consequently to a change in the clock's daily rate. Besides this, the sudden blow which the pendulum receives excites a vibration through its whole length, and gradually displaces, when suspended by a knife-edge upon a horizontal plane, the axis of motion, thus rendering it necessary to place the knife-edge in the bottom of a groove, and to give to it all the characters of a rubbing axis.

The escapement which I have contrived is calculated to remove all these inconveniences, and almost to place the going of the time-keeper beyond the reach of errors in the workmanship. The description of the escapement will be best given by tracing over the mode of its operation.

A is the pendulum rod, represented as having nearly reached the limit of its vibration to the left, and as about to touch the small friction roller attached to the arm C D of the impeller B C D E. The upper part of the pendulum rod is broken off to shew the axis B, concentric with the axis of motion of the pendulum itself, on which the impeller turns. The two axes coinciding in direction, no rubbing ought to take place though there were no friction roller at D; the roller is merely placed there for the purpose of preventing the bad effects of any small error in the adjustment. In the drawing, the weight of the impeller is represented as sustained, through the intervention of the slender spring E F, by the lifting pin F, which is placed near the centre of the scapement wheel; this wheel itself being prevented from advancing by the opposition of the detent to the detaining tooth H. The end of the spring E F is furcated, the pin resting in the bottom of the notch, and keeping the spring bent upwards from its natural position by a distance rather more than the minute diameter of the pin.



The oscillation of the pendulum is so nearly completed, that, when finished, the impeller B C D E may be lifted till the extremity of the spring just escapes from the pin F, and takes up a position a little to the left of its present one. The whole weight of the impeller now rests upon the pendulum; but when the pendulum begins to retire, the extremity of the spring is not arrested by the pin F, but passes close by it, directing its motion towards the pin G.

The impeller continues to press against the pendulum rod, and increases its momentum until the arm B E reaches a pin at L, projected from a branch of the detent H K L. After this the pendulum continues its oscillation uninterrupted.

The detent turns upon an axis at K, so that the pressure of the impeller upon the pin L elevates the detent, and allows the detaining tooth H to pass forwards.

Just at this moment the second lifting pin G is entangled between the sides of the notch in the extremity of the spring E F; the motion of the wheel, therefore, again elevates the impeller, the rise of which allows the detent to descend upon the stop N and await the arrival of the second detaining tooth I, whose arrest is announced by a distinct beat.

The whole of the escapement has now assumed a position exactly analogous to that which it had at first, and awaits the approach of the pendulum, to solicit anew its maintaining power.

During the whole of this action the pendulum is never connected with the train of wheels. The only body which acts upon it is the impeller, and this communicates to it the impulse which is generated by a descent of a constant weight through a determinate distance. The lightness of the parts renders oil either on the axis B or on the pin F unnecessary, so that this action is entirely freed from any error which might have arisen from changes in the adhesiveness of oil. In order to solicit the impulsion, the pendulum has to raise the impeller through a distance determined by the thickness of the pin F, and has to overcome the friction of the spring against that pin. But the diameter of the pin is so small, and the flexure of the spring so slight, that the errors caused by them must be exceedingly small, especially when we consider that they are not liable to any variation. The unlocking of the detent H, instead of being performed by the pendulum, is effected by the impeller; so that, however variable may be the maintaining force, provided it is never so small as to be unable to raise the impeller, nor so great as to prevent the unlocking of the detent, the going of the clock can never be in the slightest degree affected.

When the pendulum rod reaches the friction-roller, it is moving with a very small velocity, since it is almost at the limit of its oscillation, so that nothing analogous to the blow of the common 'scapements takes place; and even the sudden removal of the pressure of the impeller, when the arm reaches the pin L, can hardly excite any tremour in the pendulum.

In almost all delicate escapements, high finish in the rubbing surfaces and great accuracy in the workmanship are absolutely essential to good going. In every case the advantage of careful execution cannot fail to be felt; but in this escapement that advantage is by no means great. The execution of the train is almost a matter of indifference; and even in the most vital part, though the distances of the detaining teeth were inaccurately laid off, the errors would recur at every revolution of the escapement wheel, and their effects on the going would be generated and destroyed in the same period, so that the daily or hourly rate could not be affected.

The motion of the train resembles that of a perfect dead-beat, although the escapement certainly partakes of the nature of the recoil, since the unhooking of the spring is only effected after a slight elevation of the impeller. The beat is made only at each second oscillation, so that, in order to beat seconds, a half seconds pendulum must be used. In escapements which beat at each vibration, it is difficult to have two consecutive intervals exactly equal,—the one being less, and the other as much more than an exact second; but when the beat is given only on one side, no such inequality can exist.

The parts of the impeller are liable to expansion by heat, but the effects of this can easily be obviated by extending an arm made of some expansive metal such as zinc, on the other side of the axis B, while the branches represented in the figure are made of glass. This arm also will allow a weight to be slid along it so as to regulate the intensity of the impulse.

When the spring is released from the pin F, it does not merely assume its position of rest, but continues for a moment to vibrate on each side of it. As there might appear to be some risk of its catching again the same pin, a damper has been put on to diminish these oscillations; but, as in some other escapements which I have constructed on the same principle, it was not found necessary, it has been omitted in the drawing.

I need hardly point out to the Society, that the number of lifting pins is not limited to four, and will leave the consideration of the simplicity of the machinery, and its fitness for producing the desired end, to themselves.

On the Occurrence of the Megalichthys in a Bed of Cannel Coal in the West of Fifeshire, with Observations on the supposed Lacustrine Limestone at Burdiehouse. By LEONARD HORNER, Esq. F. R. SS. L. & E. Fellow of the Geological Society. Communicated by the Author *.

THE specimen which has led to this communication was given to me, a few weeks ago, at Dunfermline, by Mr Mackie, manager of the factory of Messrs Arthur, Aitken and Company. It is an object of considerable geological interest, being a very fine specimen of a tooth, of the same nature with those found in the limestone of Burdiehouse, near Edinburgh, which were first brought under the notice of the scientific world by Dr Hibbert. He conceived them to be the teeth of a saurian reptile; but their true nature was afterwards determined by the more experienced eye of M. Agassiz, who pronounced them to have belonged to a sauroid fish. M. Agassiz considered the fish to be a new genus, calling it Megalichthys, in reference to its great size, which the largeness of the teeth indicate; and he designated the particular species found at Burdiehouse by the name of Megalichthys Hibberti. This specimen was found accidentally in a mass of cannel coal, which they were breaking into small fragments, to be cast into a gas retort; and it is to be feared, that many precious relics of a similar nature have been destroyed by the same fate which awaited this very ancient record of the past ages of our globe.

The tooth is two inches long, and seven-eighths of an inch in diameter at its base. It is covered with a thin shining enamel, which is longitudinally striated, and, within a quarter of an inch of the base, deeply furrowed. It is not entirely circular, but is somewhat flattened. The enamel of the teeth, found in the limestone of Burdiehouse is of a pale brown colour, but this is black; the internal substance is, however, the same in both. It is in size and general appearance very similar to that figured at page 183. of Dr Hibbert's Memoir (*Transactions of the Royal Society of Edinburgh*, vol. xiii.), and is, I believe, the largest and most perfect tooth that has yet been met with in the coal itself; those hitherto found at Stoneyhill, near Musselburgh, being in general small.

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* Read at a meeting of the Royal Society, 1st February 1836.

This cannel coal was brought from Halbeath in the county of Fife, about two miles eastward of Dunfermline. I had not an opportunity of examining the locality at the time I got the specimen, the weather not being then favourable for such a purpose; but through the kindness of Mr Bowes, surgeon in Dunfermline, I was referred to Mr Geddes, mining engineer, who is intimately acquainted with the coal-fields in that part of Fifeshire, and especially with the colliery from which this specimen was obtained. He has been so obliging as to give me a description of the spot, from which I have extracted the following particulars, as more particularly bearing upon the subject of this communication.

The country around Dunfermline is composed of the stratified rocks of which the coal-measures usually consist, viz. alternations of sandstones, slate-clay, bituminous shale, which is frequently indurated, clay ironstone, and coal. There are, besides, beds of limestone, which, as seen at Charleston, appears to form the outer or high edge of the basin in which the coal-measures are situated, and at a vast depth below the bed of coal in which the fossil tooth was found. This is usually considered to be the mountain or carboniferous limestone. The alternating sandstone is of variable thickness, being in one bed as much as 102 feet, and the slate-clay varies from a few inches to several feet. The seams of coal are also of different dimensions, from five inches to seven feet. They are chiefly distinguished with reference to their economical applications; and they include both cannel coal and glance or blind coal. A section at the Halbeath colliery of 431 feet, gives 26 feet of workable coal. The general bearing of the strata is between south-east and north-west, and the lower beds have been ascertained to extend between two points which are five miles asunder. The superior beds appear to have been carried off by denudation in many places, after having been thrown up and shattered by disturbing forces, which have occasioned numerous faults. Although no trap-dikes appear, there is an overlying mass of trap in the vicinity, which, I conceive, is in all probability connected with a deep-seated dike. It is most likely that the eruption of the trap has been the chief cause of these disturbances.

The faults vary in width from 2 feet to 240 feet. In Halbeath colliery the strata are subject to five different dislocations, in a distance of about half a mile, as is represented in the annexed section in Plate III., besides other troubles, which produce similar effects on a smaller scale. The bed marked *b* is the seam of cannel coal in which the fossil tooth was found; it is twenty-three inches in thickness, the immediate roof being a slaty sandstone, and the floor an ordinary white sandstone.

It will thus be seen, that this bed of cannel coal, containing remains of a sauroid fish, is one of a regular series of alternating coal-measures of the usual characters, some of which abound in vegetable remains, which, as well as those from which the coal itself has been derived, must have been nourished during their growth by fresh water; that it is in conformable stratification with the shales containing these plants, and partakes in all the dislocations of these and the other strata.

The interest which has been excited among geologists by Dr Hibbert's researches at Burdiehouse, leads us naturally to inquire, whether the occurrence of remains of the same species of sauroid fish, in this new locality, tends to shew an analogy between the deposit at Halbeath and that at Burdiehouse? I think it does; not, however, by establishing a difference between the beds at Halbeath and those of coal-fields in general, but because I have not been able to discover any thing in the phenomena exhibited at Burdiehouse, which should lead us to consider any member of the series of strata there as having been formed in a manner different from that, which is now generally considered to be the most probable explanation of the circumstances under which deposits of coal, and the accompanying sandstones and shales must have taken place. Dr Hibbert, on the other hand, considers the deposit at Burdiehouse as an exception to the general rule, by the existence in it of a bed of limestone of peculiar characters, and which he denominates a FRESH-WATER FORMATION.

A large proportion of the stratified rocks which contain marine remains, may be said to be, in great part, of fresh-water origin; for the materials of which they are chiefly composed must have constituted the substance of pre-existing rocks, which were abraded by atmospheric agencies and running water, the detritus being afterwards transported by rivers to the sea; and in some of the beds thus formed, such as the coal-measures, the products of fresh-water are in great abundance. But this is not the sense in which Dr Hibbert employs the term: he considers the bed of limestone in question to have peculiar distinctive characters; that he has made a discovery of a new feature in our coal-fields, and one, moreover, which he had been long expecting to find. "I had long," he says, "been prepared to expect that a limestone of a fluviatile or a fresh-water origin would, some time or other, be proved to exist."—P. 169. He states (p. 267) "that it must have been the result of a deposit in fresh-water, hostile to the growth and increase of marine shells and corallines;" that this limestone bed "indicates some fresh-water river or lake, within which calcareous matter was elaborated.—P. 253. Farther, that "the beds of argillaceous shale, both above and below, enclose the same organic remains as are found in the limestone, along with coprolites, shewing that they are themselves a portion of the *lacustrine* deposit of this locality."—P. 244. And, at p. 272, he says, "Hitherto, however, I have not found the slighest traces of marine mollusca or corallines in the limestone of Burdiehouse; and hence, I am not induced to consider it as any thing but *a pure lacustrine formation*."

It is now generally admitted, as the most probable theory of the formation of coal-deposits, where there are interstratified marine beds, that they have taken place in estuaries, in those deep indentations of the land which often occur at the mouths of great rivers; and where the beds that are gradually formed, by the subsidence of the solid materials brought into it by the waters, must contain the productions both of the sea and land; those of the land, however, naturally predominating. The beds of coal are usually considered to have been formed by the accumulation of large quantities of vegetable matter, drifted into the estuary from the land, and deposited upon a previously formed surface of sand, clay, and mud, indurated afterwards into stone by pressure, and by a chemical action among the particles, induced by that enormous pressure; the vegetable matter being converted into coal by the combined chemical action of water and that same compressing force. The numerous alternations observed in the coal-measures, and the frequent intercalation of beds of limestone abounding in marine remains, indicate not only frequent changes in the nature of the materials brought from the land, but the predominance of sea over fresh water for long periods, over the areas occupied by the accumulations of transported detritus, and repeated submergence and re-elevation of the bed of the estuary.

Now, after an examination of the spot and the specimens, and after a careful perusal of Dr Hibbert's memoir, I cannot find any thing in the limestone of Burdiehouse adverse to the theory of its having been so deposited in an estuary; but, on the contrary, the evidence appears to me strongly to favour that hypothesis, and to be hostile to the idea of its being a lacustrine deposit.

Dr Hibbert himself, in speaking of the great coal-formations of the Scottish Lowlands generally (p. 258), while he makes an exception in regard to this particular bed of limestone, admits, "that even large tracts of dry land might have subsisted, and have been invaded by arms of the sea or estuaries;" and, in another place, in the summary of the evidence he adduces in favour of his theory of a lacustrine deposit, he says (p. 265), "the calcareous deposite must have taken place in a depression or basin, perfectly surrounded with a dense vegetation, which has been washed into inland waters. But this circumstance, he goes on to say, "would of itself prove little, as we may easily suppose that an estuary or arm of the sea might have stretched through a tract where a dense vegetation has prevailed." In his account of what he considers an analogous formation in Linlithgowshire, Dr Hibbert says (p. 255), "Near Bathgate, a limestone of marine origin may, at its junction with a fluviatile bed, be found to actually graduate into a fresh-water deposit." Now, this is exactly such a kind of formation as one might expect would take place in an estuary, where any of the beds might partake, in some degree, of a fresh-water character.

The evidence which Dr Hibbert considers as conclusive in favour of this limestone being of lacustrine origin is, (p. 264),

1. The absence of all mollusca and conchifera, of acknowledged marine origin.

2. In connection with the absence of marine shells, the profusion of terrestrial plants.

3. The presence of the remains of fishes that inhabited fresh water, but which Dr Hibbert admits to be an ambiguous criterion, (p. 271).

4. The abundance of the shells of entomostraca, scattered through the limestone.

Let us now examine the weight of that evidence; and first, as to the absence of marine shells.

In the immediate vicinity of Burdiehouse, there is a limestone abounding in marine remains, which Dr Hibbert describes, and which occurs in nearly conformable stratification with the other coal-measures, and with the so-called lacustrine limestone. The mere inspection of the diagram given by Dr Hibbert to shew the relative position of the two beds of limestone would lead us to conclude that they were deposited in the same waters, and belong to one series; and we know that nothing is more common than to find, in a series of strata, some beds of limestone containing organic remains, and others in which not a trace of an organized body can be discovered. Near Lulworth, in Dorsetshire, where the Purbeck beds are largely developed, and which abound in organic remains, there are compact varieties of Purbeck stone, which are devoid of shells, and which attain a thickness of from 60 to 100 feet.* Many of the beds of the lias and oolite series of limestones, and which alternate with shales and sandstones, are almost wholly made up of organic remains, while others of the same series are wholly destitute of them. The same thing has been observed in the carboniferous limestone of Wales, of the north of France, and of Belgium. Marine *shells* may not, as yet, have been discovered in the limestone under consideration, but marine *organic remains* are abundant in it, as I shall presently shew. But even beds containing exclusively fresh-water shells, in the opinion of geologists of great authority, do not afford conclusive evidence of a lacustrine deposit. In the memoir of Professor Buckland and M. De La Beche, on the Geology of the Neighbourhood of Weymouth,† the authors observe, " One of the most important points in the geological history of the Purbeck series, is the occurrence of a bed of oyster-shells, called the cinder-bed," often many feet in thickness, and almost wholly composed of dark-coloured small oyster-shells in the midst of a series of strata, some of which contain exclusively shells of fresh-water formation, and others an admixture of fresh-water shells with those which are marine; and although we cannot infer from it the return of the sea for any long period in the middle of the Purbeck formation, yet it shews that the district it occupies could not have been a lake of pure fresh water, but was probably an estuary at the time when these oysters occupied its bottom, and were accumulated to the thickness of many feet over a distance of many miles." The same authors add a note descriptive of the Lake Menzalé, at the mouth of the Nile, which, they remark, "is highly illustrative of the mode in which living animals, of a mixed character, are associated together near the confluence of great rivers with the sea."

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* Geol. Trans. 2d ser. vol. iv. p. 12.

† Ibid.

2dly, As to the plants. All the species of plants which have been found in this limestone have been met with in the shales and sandstones of other coal-fields, either of this country or of the Continent. The *Sphenopteris affinis* which, as Dr Hibbert states, occurs in greatest abundance in the limestone, is common in the roof of the Bensham coal-main in Jarrow colliery, near Newcastle;* and the Lepidostrobus variabilis, of which a specimen from the limestone is figured by Dr Hibbert, associated with a fish of the genus Palæoniscus, which I shall afterwards shew must have lived in the sea, is also met with in Jarrow colliery.† But these plants are not confined to the coal-measures; but are met with throughout the whole carboniferous series, from the old red to the new red sandstone. M. Elie de Beaumont describes the graywacke rocks, at the extremity of the Vosges Mountains in Alsace, and of the Bocage in the department of Calvados, a part of ancient Normandy, as containing vegetable impressions scarcely differing from those found in the coal-formations.‡ They are by no means uncommon in the carboniferous limestone; and I have seen in the collection of Professor Jameson, specimens collected by him near Pettycur in Fifeshire, of a coarse limestone belonging to the coal-measures containing the same class of plants.* It is clear, therefore, that the mere existence of terrestrial plants does not prove a lacustrine deposit.

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* Fossil Flora of Great Britain, plate 45. † *Ibid.* plates 10 and 11.

‡ Phil. Mag. and An. vol. x. p. 247.

3dly, As to the remains of fish. These are the Megalichthys, Pygopterus, Amblypterus, and Eurynotus, and are supposed to have approached the cestracion of modern times.

Dr Hibbert considers the Megalichthys as a fresh-water fish, in one part of his memoir, for, in describing the circumstances under which he conceives the coal deposits of Scotland to have taken place, he says, "During such a condition of the globe, the calcareous deposit of Burdiehouse was formed, new races of fish inhabiting *fresh waters* were created, *and among them the Megalichthys*."—P. 258. And, in another place, he says, "As the remains of the Megalichthys are found in bituminous shale, and even in coal itself, it is evident that the animal must have frequented shallows and wet marshes."—P. 262. He points out the analogy, observed by M. Agassiz, between the Megalichthys and the recent Lepidosteus; speaks (p. 207) of the Lepidosteus Spatula as being "a living type of the Megalichthys; and states (p. 213) that the Lepidosteus dwells among the lakes and rivers of the most thermal regions of America. In speaking of the coprolites, however, he makes use of some expressions which would seem to indicate a different view, viz. that this great fish must only have been an occasional visiter of fresh water. He says, "In proportion as coprolites increase in size, we find that they contain the scales of fish, shewing that the larger fish, to which these fœcal remains are referred, must have frequented the ancient river or lake, indicated by the limestone of Burdiehouse, in quest of their prey." Now, this is obviously quite inconsistent with the idea of a "pure lacustrine formation," for when he speaks of large fish frequenting the ancient river or lake in quest of their prey, he obviously means that they were not regular inhabitants of the river or lake; and as we must presume that they came from the sea, and must have swam into the lake, it must therefore have communicated with the sea. But there is a passage in Dr Hibbert's memoir which

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* For an account of Professor Jameson's discoveries in this locality, see Proceedings of Wernerian Natural History Society, in Edinburgh Philosophical Journal, January 1836.

I am quite at a loss to reconcile, either with his statement that the Megalichthys was a fresh-water fish, or with his theory of a "pure lacustrine deposit." He says, p. 271, "As for the remains of Cestracientes (*and perhaps of the Megalichthys*), which appear in more than one description of carboniferous limestone, they point to estuaries, no less than to fresh-water lakes, as having been, in primeval times, frequented by large animals in quest of prey."

M. Agassiz, in his memoir on the Geological Distribution of Fossil Fishes, read before the Geological Society on November 1834, states that "he cannot, on ichthyological data, decide on the fresh-water or marine origin of the fish of the ancient groups." There is, therefore, no evidence afforded by the remains themselves, either of the marine or the fresh-water habits of the Megalichthys; but we may infer that M. Agassiz inclines to the opinion of its having been a sea fish, from what he says in his "*Rapport sur les Poissons Fossiles découverts en Angleterre.*" In speaking (p. 28) of the Megalichthys Hibberti of Burdiehouse, he says, "Ces fossiles proviennent d'un poisson d'une famille qui ne comprend que deux genres dans la creation actuelle; dont les représentants peuplaient surtout *les mers* qui recouvraient la terre, avant la déposition des terrains crétacés; famille que j'ai appélée celle des sauroïdes." Dr Hibbert quotes Cloquet's article in the *Dict. des Sc. Nat.*, when he describes the Lepidosteus as an inhabitant of the lakes of South America. But Cloquet is then speaking only of the two species, L. Gavial and L. Spatula. In describing the other species, the L. Robolo, he says, "on peche ce poisson dans la mer qui arrose le Chili—les insulaires de l'Archipel de Chiloe font secher à la fumée une grande quantité de ces robolos, et en font un commerce étendu." Dr Hibbert ought, therefore, to have shewn that the megalichthys has a closer affinity to the fresh-water than to the marine species of lepidosteus, before any conclusive argument can be drawn from the resemblance.

Thus it is evident, that the remains of the megalichthys afford no evidence whatever of a lacustrine deposit, while their occurrence in the regular coal-beds at Halbeath, and in those of Stoneyhill near Musselburgh, the neighbourhood of Glasgow, and other places, tend to prove a similarity of formation between the supposed fresh-water limestone and the other coal-measures.

The other sauroid fish, remains of which have been found at Burdiehouse, is the Pygopterus. Now, most of the specimens of this genus of fish which have hitherto been met with, have been derived from strata abounding in marine fossils, viz. the Zechstein of Mansfield and other places in Germany, and the magnesian limestone of the county of Durham; they have also been found in the coal formation at Saarbrock;* and M. Agassiz has recognised, in the above mentioned limestone of the coal-measures at Pettycur, a new species which he has named P. Jamesoni.

" The fish," says Dr Hibbert, " which the limestone entombs in far the greatest number, is an individual which I had little difficulty in referring to the genus Palæoniscus."—P. 190. Now the genus Palæoniscus is found abundantly in the Zechstein of Mansfeld, and in the equivalent of that rock in England, the magnesian limestone at East Thickly in the county of Durham.† In this last locality the remains of this fish are associated with vegetable impressions which Professor Sedgwick refers to the fern tribe,‡ and with an impure coal. They have been met with, besides, in different coal-formations in England, France, Germany, and the United States; and three species have been recognised in the limestone of Pettycur, one of which, P. Robisoni, is identical with that which is found in such abundance at Burdiehouse.

Of the five species of Amblypterus described by Agassiz, four are from the regular coal deposits of Saarbrück and that neighbourhood, the other being from Brazil, but in what formation it is not mentioned.

The Eurynotus is said by Dr Hibbert, p. 192, to resemble the Platysomus. M. Agassiz describes five different species of the Platysomus, and of these, two were obtained from the Zechstein, and three from the magnesian limestone. He has found a species of Eurynotus in the limestone of Pettycur.

It appears, therefore, that the fish found in the limestone under consideration, in place of being an " ambiguous criterion," clearly indicate that the bed in which they are found must have been deposited in salt or at least brackish water, and not in a fresh-water lake.

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* Agassiz, Poissons Fossiles.

† Ibid.

‡ Geol. Trans. 2d ser. vol. iii.

Apparently the strongest argument, which Dr Hibbert has brought forward, in favour of his theory, is the great abundance of the shells of microscopic animals, *entomostraca*, which are scattered through the substance of the limestone; and which he considers to belong to the fresh-water genus Cypris. Now supposing him to be correct in this, it is by no means a conclusive proof of a lacustrine deposit; for the animals may have lived in marshes or stagnant waters, such as are common near the mouths of great rivers, and have been washed into the estuary during floods. But it is not at all clear that these shells are really fresh-water. The similarity between the shells of the Cypris and those of the Cytherina of Lamarck was long ago pointed out by Müller. This is a marine genus of entomostraca; and Müller, in describing it, says, "Species *variæ* in Fucis et Confervis marines degunt, in flustris, præsertim in lineata, delitere amant;"* and Lamarck says that they inhabit the seas of the northern latitudes.† I am informed by Mr Lyell that Mr Lonsdale has recently discovered abundance of those microscopic shells in chalk, mingled with marine zoophytes and testacea; and he adds, that if they had been met with in the fresh-water deposits of the Wealden, they would undoubtedly have been called Cypris.

Dr Hibbert observes (p. 225.), that, "in the diffusion of the vegetable and animal remains through the limestone, little or no order is preserved. Vegetable and animal remains are not confined to particular seams of the rock, but may occur in any part of it. Nor are they confined to the limestone itself, since they have been found in argillaceous and bituminous shale both above and below the bed." Now this is surely very unlike that tranquil deposition which we find so generally characteristic of lacustrine formations; but it is very like that more disturbed state which we might expect to find in the waters of an estuary, agitated by the continued flow of a river, and by the motions of the tides.

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* Otho. Frid. Müller, Entomostraca, Lipsiæ, 1785, 4to, p. 64.

† Lamarck, Animaux sans Vertebres, v. 125.

Upon a review, therefore, of the whole evidence, it appears to me, that there is nothing to warrant us in considering the limestone at Burdiehouse as a pure lacustrine formation; that the series of coal-measures there are different in geological characters from other series of carboniferous deposits; or that the limestone bed in question was formed under conditions different from those of the shales, sandstones, ironstone, and seams of coal with which it is associated.

In thus freely expressing my doubts of the soundness of the conclusions to which Dr Hibbert has arrived, I trust that I have not exceeded the limits of fair scientific criticism; and I farther hope, that nothing which I have said can be construed as inconsistent with a just admiration of the industry and zeal displayed by him in these researches, or with the respect that is due to him, for his many valuable contributions to science and literature.[*]

Remarks on the Dublin and Kingstown Railway, intended as a Supplement to a former Paper on the Liverpool and Manchester Railway, in the 18*th Volume of this Journal*, 1835. By DAVID STEVENSON, Esq. Civil-Engineer, Edinburgh.[†] With a Plate.

SINCE my paper on the Liverpool and Manchester Railway was laid before this Society, in the month of February last, I have, in the course of my professional pursuits, visited most of the public railways of the United Kingdom, and, in connection with this subject, I also paid a visit to some of the great iron-works in Wales.

The application of tram-roads and wooden railways to the conveyance of coal and other mineral products, was introduced in the neighbourhood of Newcastle so long ago as the sixteenth century; and this species of road, although it possesses many disadvantages, is still in use in some of the old mining districts, both of England and Scotand.

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[*] Since this paper was read, I have seen the fifth Livraison of the work of M. Agassiz, "Sur les Poissons Fossiles," in which particular mention is made of the researches of Dr Hibbert at Burdiehouse. I have not found any observation of M. Agassiz at variance with the opinions I have ventured to express, and I observe, that, in speaking of the Sauroides, he specially calls the attention of his readers to his opinion, that they do not form a family intermediate between ordinary fishes and reptiles, adding, "En effet, mes Sauroides sont de vrais poissons; ce sont les premiers poissons voraces qui aient vécu *dans les mers* d'autrefois."

8th March 1836.

[†] Read before the Society of Arts for Scotland, 9th March 1836.

At Colebrookdale Iron-Works in Shropshire, Mr Reynolds, the proprietor of these works, in the year 1767, first substituted the metallic plate railway for the wooden road, a most important era in the history of what has appropriately been termed the "British Roadway." Emboldened by the success which attended the introduction of the cast-iron railway, it was the same person who, in the year 1777, erected over the river Severn the first cast-iron bridge constructed in this country.

A great improvement was undoubtedly effected in the construction of railways, by the introduction of the cast-iron rail. However, from the brittle nature of that material, it was soon found to be very unfit for giving support to the great weights which pass along railways. Accordingly, in the year 1811, a malleable iron railway was constructed at Lord Carlisle's coal-works in Cumberland, which forms another important era in the history of the railway, and this system was first publicly noticed in my father's Report of the Edinburgh Railway in the year 1819. Malleable iron has been more or less used since that date, and is now universally employed with the greatest success in the construction of railways. Indeed, it must be obvious, that the speed at which we now travel, and the liability of cast-iron rails to break, render them quite inapplicable to the improved state of railway conveyance.

Merthyr Tidvil in Glamorganshire, which I visited on my way from Holyhead to Plymouth, is by far the greatest iron district in the kingdom. Here I found the extensive works of Mr Guest, Mr Crashey, and others, directing their whole resources to the manufacture of malleable iron rails for almost all parts of the world. What a striking change in the arts presents itself to our observation, when we consider that it is no more than thirty-five or forty years since the attention of the engineer was wholly engrossed in the formation of canals; and Europe and America were without an iron railway excepting that of Colebrookdale in Shropshire.

Fig. 1. On examining these blocks, I found many of them split, caused no doubt by undue pressure, arising from the difficulty of procuring a solid bed for so large a stone. Mr Vignoles, it is believed, has recommended their removal, and the substitution of the common insulated block. The object in adopting this sort of block was to form a road as perfectly rigid or inflexible as possible. It is also useful in preventing the rails from being separated, which, especially on sharp curves, is apt to take place. This connection between the rails on the Newcastle and Carlisle Railway is formed by means of a bar of malleable iron, with a cheek formed at both ends as a seat for the rails, while the bar itself rests on the stone blocks, as shewn in Plate IV. Fig. 2. In this way, the objections arising from the expense of procuring large blocks of stone, and their liability to break, are obviated. On the Dublin and Kingstown Railway, they are much troubled by the tendency which the chairs have to shake loose from the granite blocks; to counteract which, the use of *felt*, *wood*, *lead* and *copper*, has been applied as a bedding for the chair, but with little effect. The rails of the Liverpool and Manchester line are more easily kept in repair; here, freestone blocks, measuring two feet square, are used for supporting the rails, but the mode of fixing these to the chairs is more simple than in the Dublin and Kingstown Railway. The method, however, of fixing the chairs to the blocks is the same in both cases. The difficulty experienced in keeping the Dublin and Kingstown Railway in repair may arise in a great measure from the rigidity of the rails, produced by the unyielding nature of the granite blocks. Between Liverpool and Manchester, the part of the road requiring least repair is that over Chatt Moss, where the railway may be said to float on the surface of the bog. The motion of the trains in passing over this part of the line is also sensibly retarded. The weight of the train causes a depression or hollow in the road, which offers the same resistance as a gentle inclined plane, to the progress of the engine. This is a good practical proof that a flexible railway offers more resistance to the motion of a carriage passing along its surface than one which is in a more rigid state, while it possesses the advantage of being much more easily kept in good repair.

When the curves on this line of railway are of small radius, the external rail is raised a little above the level of the internal one, as shewn in an exaggerated scale in Plate IV. Fig 3. On the curve of half a mile radius at Kingstown the difference of level between the two rails is one inch. This is certainly good in theory, and may serve to check the centrifugal force, which in a body moving rapidly round a curve of so small radius must be considerable. The raising of the external rail on curves is not peculiar to the Dublin and Kingstown Railway, and has been introduced with good effect on different works. The trains run round the curve of one half mile in radius, at the rate of twenty miles an hour, and no accident has ever happened. On the Liverpool and Manchester Railway the curves are not so sharp as to render this precaution at all necessary.

A great improvement has been effected in the working of the carriages at the Dublin and Kingstown Railway, by the application of spiral springs to the *buffing*-apparatus of the carriages, for softening their collision; as suggested by Mr Bergin of the Railway Company. These spiral springs are about three feet in length, and consist of an ingenious combination of shorter springs, varying in strength. By this means, when a carriage strikes gently on any obstruction, the weaker part of this combined spring is alone affected,—and when the collision is more violent, the stronger parts are brought into action. This arrangement has rendered the shocks formerly felt in starting and stopping the carriages much more gentle, and is certainly a valuable and highly useful application of the spiral spring.

The locomotive engines used on the Dublin and Kingstown Railway were made in England. Several of them have vertical cylinders, which both here and at Liverpool have not been found to act so well as those in which the cylinders are horizontal. One engine on the Kingstown line has been constructed to carry its own fuel and water, and thereby dispenses with the use of a tender. This engine, with its apparatus, weighs about twelve tons, and I believe acts very well.

I beg in conclusion to remark, that, with the exception of the peculiarities now mentioned, the observations which I formerly made to the Society, on the details of the Liverpool and Manchester Railway, are generally applicable to the railway between Dublin and Kingstown.

Single Reflecting Microscope. By ALEXANDER GUTHRIE, Esq. Communicated through J. ROBISON, Esq. Sec. R. S. E.

THE superiority of single reflection, in adapting it to the reflecting telescope, for the reception of much greater magnifying powers, and for its affording a greater degree of additional light and distinctness, has been justly appreciated; but, while every other plan of the reflecting telescope has been metamorphosed into the reflecting microscope, that plan alone, which is the best adapted and the most simple, has been allowed to escape.

Let ABCD, Fig. 1, Plate III., represent a tube furnished with a field-glass *m*, an eye-glass *n*, and an eye-hole *p* at the one end; and on the other a ring A*a* D*d* soldered, with its axis coincident with that of the tube. This ring is seen in section, in fig. 1; but fig. 3. is a ground plan of it. EF, a circular plate of the same dimensions as the ring, and attached to it by three equal and equi-distant columns, with its plane parallel to that of the ring; E*a* represents one of these columns, but the whole three are seen in Fig. 2. Let GH (Fig. 1.) represent a concave speculum, set on the circular plate EF, with its axis coincident with that of the tube.

The rays of light from an object placed in the focus of parallel rays, will, after reflection, pass on parallel to one another; but, if the object be removed back from the speculum to a certain point O, the rays after reflection will converge, and form a magnified image of the object at I, which can be viewed to advantage from the eye-hole *p* by the eye-glass *n*.

In order to place the object in the axis of the speculum, and to adjust the focus, let *ab* (Fig. 5.) represent a segment of a circle, of the same dimensions as the ring (Fig 3.), having a hole *a* in the one end, of sufficient capacity to run freely on one of the columns; and a hole *b* in the other end, with a female screw in it, and having a small spring *d* attached to it by rivets at the one end of the spring; and let *xy* represent the instrument on which the object is fixed, which can be slipped in below the spring in any position. Let *ef* (Fig. 4.) represent the same segment in perspective, the female screw *f* receiving the spindle *a b*, having a part of it screwed, and having a milled-nut *c* upon it. Let the pivots *a b* of the spindle be transferred to the holes *a b* Fig. 2, with one of the columns passing through the hole *e* of the segment, and the instrument is completed.

To use the microscope, let the instrument be placed on the instrument *x y*, and let that instrument be so placed that the object shall be in the axis of the speculum, or, which is the same thing, that its image be in the field of view; and let the spindle be turned in any way necessary for the perfect adjustment of the focus, by the friction of the finger upon the milled-nut of the spindle, which thus causes the object traverse in the axis of the speculum, and the focus is thus adjusted with the utmost precision.

Instead of having the tube ABCD, Fig. 1, all in one piece, the instrument is much improved by having it in sections to draw out of one another, as shewn in the figure. It thus gives different degrees of magnifying power by the same eye-glass, in proportion to the length to which it is drawn out. And when it is shut up, the tubes, by covering the speculum, render the instrument quite portable.

The magnitude of the image, compared to that of the object, is in the proportion of their respective distances from the speculum; therefore, by dividing the distance IH, Fig. 1, by the distance OH, and multiplying the quotient by the magnifying power of the eye-glass, * the magnifying power of the instrument is obtained.

Notice of some minute Calculi found in the Urinary Bladder of an Ox. By JOHN DAVY, M. D. F. R. S., Assistant-Inspector of Army Hospitals, &c. Communicated by the Author.

THE calculi of which I propose to give a short account, were presented lately to the Museum of the Medical Department of the Army by Dr Williams, Surgeon of the 68th Regiment.

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* The magnifying power of the eye-glass is found by dividing eight inches (the distance of the object from the eye in perfect vision), by the focal distance in inches of the eye-glass.

He informed me, that he had procured them from a butcher, by whom they had been found in the urinary bladder of a healthy ox, killed for the market at Portsmouth.

They were about fifty in number. The largest was little larger than a grape seed; it weighed $\frac{8}{10}$ths of a grain;—the smallest were exceedingly minute, less in size than the finest mustard seed, and weighed less than $\frac{1}{100}$th of a grain. They were all of a pearly lustre; externally of a yellowish-brown hue; internally of a silvery white. The smallest were spherical; the forms of the larger were less regular, they were imperfect spheres. Their structure was concentric lamellar.

Before the blowpipe, they decrepitated with explosive violence. When heated, confined between folds of platina-foil, so that their minute fragments could be collected, they (the fragments) were found to be portions of very fine laminæ, which blackened when farther heated, and ultimately became perfectly white. When heated in a glass tube, water was collected; to the rapid conversion of which into steam probably the violent decrepitation was owing, at least in part. I say in part, because on one occasion two or three of their calculi decrepitated, at a comparatively low temperature, when placed before a fire in a small glass jar, covered with tin-foil and varnished, (for the purpose of rapidly drying the varnish, preparatory to placing it on the shelf in the Museum),—giving the idea, that electricity might be concerned in the phenomenon.

The ash obtained from them, after the action of the blowpipe, was bulky; it effervesced powerfully in dilute muriatic acid, and was entirely dissolved. The solution was not distinctly rendered turbid by *aqua ammoniæ;* it was copiously precipitated by the sesquicarbonate of ammonia.

The calculi, before incineration, effervesced more slowly in the same acid; and, when the effervescence had ceased, they had lost their opacity; the undissolved residue, unaltered in form, was transparent animal matter.

From the results of these experiments, it would appear that the principal ingredients of these calculi are carbonate of lime and animal matter, the former greatly preponderating. As the few trials to which I considered it right to limit myself were made on some of the smaller concretions, I did not attempt to ascertain either the exact proportions of their constituent parts, or the precise nature of the animal matter; probably it was albumen.

The resemblance of these calculi to pearls (the calculi of the oyster) is very striking; and their composition being so very similar, gives them an additional interest; they might indeed without impropriety be called pearls.

FORT PITT, CHATHAM, 1*st March* 1836.

On the Cause of the Temperature of Hot and Thermal Springs; and on the bearings of this subject, as connected with the general question regarding the Internal Temperature of the Earth. By Professor GUSTAV BISCHOF of Bonn. Communicated by the Author.[*] 76

PART FIRST.—*What Thermometrical Circumstances on the Surface of the Earth lead us to assume that an Increase of Temperature towards the Centre of the Earth must take place?*

WE are indebted to Alexander Von Humboldt[†] for an ingenious inquiry into the principal causes of differences of temperature in the earth. In one of the following chapters we shall endeavour to ascertain to what depth the influence of the external temperature which occasions those differences continues to be felt. Beyond that boundary other circumstances of temperature present themselves, which are no longer connected with the geographical and physical climates; and at certain depths, which, however, are not the same in all parts of the earth, we find the same degree of temperature beneath the perpetual ice and snow of the polar regions, as under the torrid zone. That thermometrical phenomena in the interior of the earth are totally independent of all external circumstances, proves itself not only in the earth's crust, but also in the depths of the sea and of lakes, where a temperature prevails which is equally uninfluenced by them. Quantities of water springing out of the earth at almost every point, shew themselves equally independent of the influence of the external temperature, or at most but slightly modified by it. The object of this part of the present memoir will be an investigation into these phenomena, and into the circumstances more immediately connected with them, which may indirectly lead us to the conclusion, that there exists in the interior of the earth a temperature which increases progressively with the depth.

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[*] The "Societé Hollandaise des Sciences" at Haarlem, offered a prize for an Essay on the Temperature of the Interior of the Earth and of Springs, which was gained by Professor Bischof. The memoir now given to the public through this Journal, presents, in an altered and improved form, an account of the experiments, observations, and reasonings, which obtained from the Haarlem Society the high honour just mentioned.

I have, says Professor Bischof, to express my grateful sense of the assistance rendered by my talented young friend Mr Alexander Mornay, in the translation and revision of this essay.

[†] Poggendorff's Annalen, vol. xi. p. 1. and fol.

Chap. I.—*On the Circumstances under which Warm Springs are found on the Surface of the Earth, and on their frequency of occurrence.*

If we were obliged to confine ourselves to those commonly called warm or hot springs, it would be presumptuous to conclude from their temperature that of the interior of the earth in general, as they are comparatively of rare occurrence; but if we be allowed to consider every spring as warm or thermal, whose temperature exceeds, by however little, the mean temperature of the place at which it rises, we shall find that thermal springs are far from unfrequent.

It may be considered almost as a general rule, that those springs which are usually termed mineral springs exceed, more or less, the mean temperature of the place, and it must be regarded as an exception when this is not the case.

For several years I have been observing, at different seasons, the temperature of about twenty mineral springs in the vicinity of the Laacher See, and I find that even the coldest among them always exceed the mean temperature of the place by nearly 1° R. (= 2¼° Fahr.) Some of them even rise to 53°.375 F., 57°.875, and 58°.325,* and shew a constant degree of heat. The warmest of the numerous mineral springs in the volcanic Eifel, is that of the baths at Bertrich, the temperature of which is 90°.5. The hot springs of Aix-la-Chapelle and Burtscheid

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* All the temperatures given in this article are reduced to the scale of Fahrenheit.

(Borcette), attain a much higher temperature; the warmest among the latter shews 171°.5.

The numerous mineral springs of the Westerwald and the Taunus Mountains, surpass the mean temperature of the place with but few exceptions. Some, as Selters, have 60°.125; Schlangenbad, Ems, and Wiesbaden rise even as high as 84°.875, 131°, and 147°.875. Wille* determined the temperature of thirty groups of mineral springs between the Taunus and Vogelsgebirge, and found that they must be considered as thermals, with but few exceptions. The salt springs at Sooden, near Höchst on the Main, and in the valley of the Nahe, have a temperature of 74°.75 and 81°.5, and those of Nauheim even 86°.

A great number of fresh-water springs, which rise out of the chalk formation on the western declivity of the Teutoburger Wald and the Haar in Westphalia, possess a degree of heat exceeding the mean temperature of the place. According to my observations, made in April 1833, and repeated in May 1834, on the springs of the Lippe, Jordan, Pader, and Heder, on the fresh-water springs of Gesecke, Erwitte, &c., their temperatures vary from 47°.75 to 59°.25. Those whose temperatures are near 47°.75, can alone be considered as being equal to the mean temperature of the place; the warmer ones are evidently thermal, and their number is not small; for example, at Paderborn, out of sixty of the springs which I observed, there are fifty whose temperatures are above 50°, and which must consequently be considered as thermal. A warm spring, lately discovered, about forty feet distant from the Lippe springs, has a temperature as high as 69°.35. † The temperature of the salt springs of Westphalia is between 52°.25 and 63°.5. ‡

The mineral springs of other parts of north-western Germany in Hessia, Hanover, &c., for the most part also exceed the mean temperature of the place. § The salt springs in the Prussian dominions between the Elbe and the Rhine have temperatures from 52°.25 to 62°.175, and for the most part have been found not to have varied during twelve years of observation.

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* Geognostische Beschreibung der Gebirgsmassen zwischen dem Taunus und Vogelsgebirge, &c. Mainz, 1828, p. 100, and fol.

† Bischof ueber die merkwürdigen Quellenverhältnisse des westlichen Abhangs des Tentoburger Waldes im neuen Jahrbuche der Chemie und Physik, vol. viii. p. 249.

‡ Kollmann in "Das Gebirge in Rheinland Westphalen," von Nöggerath vol. iii. p. 56.

§ Osann's Physikal. Médicin. Darstellung der bekannten Heilquellen der vorzüglichsten Länder Europa's, part ii.. 1832.

In the Erzgebirge, in the Riesengebirge, and in the Bohemian Mittelgebirge, the temperature of the mineral springs also exceeds, in general, the mean temperature of the place. The Wiesen, or Jobsbad, near Annaberg, has a temperature of 70°.25; the Wolkensteiner Bad, 83°.75; Landeck, 66°.875 to 83°.813; Warmbrunn, 95° to 99°.5; Toeplitz, 79°.25 to 117°.5; and the celebrated Carlsbader Sprudel, 164°.75.

Moravia also offers several examples of warm springs; for instance, the sulphurous waters of Ullersdorf, which have 88°.25. Hot springs are very abundant in Hungary, Transylvania, Sclavonia, and Croatia; the hottest of them attain 88°.25 to 144°.5. There are a great many warm springs on the Caucasus, several of which almost reach the boiling point.*

In the interior of Germany, in Bavaria, and Würtemberg, there are comparatively few hot springs, commonly so called; there are, however, many which may be considered as thermal. The Ludwigsbad, near Wipfeld, has 56°.75; the salt springs at Reichenhall and Kissingen, 56°.75 to 69°.125; the mineral springs at Canstadt, 63°.5 to 68°.0; the Liebenzeller Bad, 76°.325; and the Wildbad, 88°.25 to 97°.25. The Grand Duchy of Baden also possesses several thermal springs, among which Baden-Baden, having a temperature of 128°.75 to 153°.5, is particularly remarkable.

A great number of thermal springs are found in the Alps. In Krain, the temperature of the Toeplitza rises to 97°.813; that of the Baths of Montefalcone, near Trieste, to 100°.625. In Carinthia, the hottest is the Villacher Bad, having 79°.25; in Styria, the Römerbad at Tyffer, and the baths at Neuhaus, of 43°.875 to 98°.825; in the Archduchy of Austria, the sulphurous warm spring at Baden, of 83°.75 to 99°.5; in Salzburg, the celebrated Gasteiner Wildbad, of 99°.5 to 117°.5.

In Tyrol thermal springs are numerous. According to the observations made, at my request, by my friend Professor Ennemoser on about twenty-six warm springs, their temperatures fall between 52°.25 and 72°.95. Neither are they of uncommon occurrence in Switzerland; the hottest are St Gervaise, near Mont Blanc, 61°.875 to 97°.925; Pfaeffers, 99 .5 to 100°.625; Aix-les-Bains, (in Savoy), 111°.875 to 116°.825; and Leuk, 97°.25 to 124°.25.*

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* Hermann Untersuchungen der Mineralquellen am Kaukasus, &c.—in the Nouveaux Mémoires de la Soc. Impériale des Naturalistes de Moscow, vol. ii. p. 387.

In France, from the Pyrenees to the mountains of the Vosges, a great number of warm springs are to be met with. Among the many sulphurous springs in the department of the East Pyrenees, which are dispersed over fourteen districts, Anglada† observed the temperature of forty-two thermal springs, of which only three possess a temperature below 77°., twelve between 77°.0 and 99°.5, ten between 99°.5 and 122°.0, fourteen between 122°.0 and 144°.5, and three between 144°.5 and 172°.625. Even the coldest among all those sulphurous springs are sensibly warmer than the neighbouring fresh water springs, which have only 47°.75 and 50° of temperature.

In the volcanic mountains of Auvergne and the Vivarais, a great many thermal springs are found. The hottest of these are: Mont-Dore-les-Bains, of 113°.0; St Nectaire, 65°.75 to 89°.6; Vichy, 90°.5 to 113°.0; and Chaudes Aigues, 190°.4.‡

As the object here is only to point out the occurrence of thermal waters in the most various formations, I think it unnecessary to mention further the hot springs of other countries, where, however, it would be easy to shew that they are to be found in equal abundance.

But it is not Nature alone that furnishes us with hot springs; art can also draw them forth from the interior of the earth. Almost all artesian wells possess a temperature superior to the mean temperature of the place. Thus the temperatures of forty-eight springs bored for in and near Vienna, were found by observations made in November 1830 to fall between 52°.25 and 57°.2,[*] whereas the mean temperature of Vienna is 50°.81.[†] At Erlangen (mean temp. between 47°.75 and 50°.0), springs of 53°.375 were met with in the Keuper (?) formation at depths of 168 and 99 feet.[‡] At Würtzburg (mean temp. 50°.675), an artesian well was sunk 200 feet deep, the temperature of which was 54°.725.[§] At Nürnberg (mean temp. between 47°.75 and 50°.0), springs were discovered in the Keuper at depths of 71, 166, and 357 feet, having temperatures of 50°.0, 52°.25, and 56°.188; and others at 100 and 318 feet of 53°.375 and 54°.5.[||] In Würtemberg a great number of artesian wells have been sunk with various success, all of which have temperatures exceeding the mean temperature of the places at which they are situated.[¶] Thus eight of these wells at Berg, near Stuttgard, in the muschelkalk yielded, at depths of 117 and 163 feet, mineral waters of 65°.75 to 70°.25, very strongly impregnated with carbonic acid gas. The warmest contained a greater, and the coldest a smaller, proportion of gas. In another case, in the same neighbourhood, a stream of water suddenly burst forth at a depth of 98 feet, with great violence, caused by the explosion of the compressed gas. The water contained a great quantity of carbonic acid, and had a temperature of about 65°.75. In and about Canstadt, water strongly impregnated with carbonic acid gas was obtained, having a temperature of 66°.425. Five artesian wells, sunk at Heilbronn, on the Neckar, yield water of 54°.5.[**] The mean temperature of the places in Würtemberg falls between 47°.75 and 50°.0. In the Amalienbad at Langenbrücken, in Baden, a strong sulphurous spring of 56°.75 was obtained by boring.[††] According to my observations on ten artesian wells at Münster, in Westphalia, their temperatures seem to exceed the mean temperature of the place by about 2°.25. Some springs bored for in the East Pyrenees also exceed the mean temperature of that country by several degrees; thus one at a depth of 80 feet had 60°.35, exactly the same as the running springs in the neighbourhood; a second, at a greater depth, shewed 62°.825.* Lastly, at Rüdersdorf, near Berlin, in the chalk formation, water of 74°.3 was obtained at a depth of 880 feet, 770 feet below the surface of the sea.†

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* Robert Bakewell (Philosoph. Magazine, January 1828, p. 14 to 59), communicates some very interesting remarks on the frequent occurrence of hot springs in the district of the pennine Alps. With these we may compare the observations of Pallasou on the numerous hot springs of the Pyrenees. (Mémoire pour servir à l'Histoire Naturelle des Pyrénées, 1815, p. 435.)

† Mémoires pour servir à l'Histoire Generale des Eaux Minérales Sulphureuses et des Eaux Thermales; Paris, vol. i. p. 31 and 35.

‡ Bischof, die vulcanischen Mineralquellen Deutschlands und Frankreichs. Bonn, 1826, p. 213.

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[*] Die artesischen Brunnen in und um Wien von Jacquin und Partsch. Wien, 1831.

[†] Kastner's Archiv. für Chemie und Météorologie, vol. iv. p. 48.

[‡] Ibid. vol. ii, p. 228.

[§] Ibid. vol. iv. p. 374.

[||] Von Bruckmann ueber artesischen Brunnen. Heilbronn, 1833, p. 224 and 229.

[¶] Correspondenzblatt des Königl. Würtemb. landwirthschaftl. Vereins 1833, vol. ii. No. 2. p. 152-1.

[**] Von Bruckmann, p. 34.

[††] Ibid. 248.

This comparison of the temperatures of springs shews that thermal waters are to be found in all formations very abundantly. They are found in the youngest members of the secondary series, as well as in the oldest neptunian and in volcanic rocks. We find them below the level of the sea, a few hundred feet above it, and at heights of 2000 to 12,000 feet.‡ Warm springs are also found in all latitudes: in the polar regions, as in Iceland and in Greenland,* as well as in the temperate zone and under the equator. The heat which accompanies them cannot, therefore, be sought in any particular formation of rocks, nor in local chemical processes, but must be everywhere distributed throughout the interior of the earth. We must necessarily be brought to this conclusion, so soon as we succeed in proving that it cannot possibly result from any chemical processes.

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* Bulletin de la Société Geolog. de France, vol. iv. p. 214.

† Poggendorff's Annalen, vol. xxviii. p. 233.

‡ I only mention a few warm springs as examples: Ems lies 291 feet above the level of the sea, Wiesbaden 328 feet, Aix-la-Chapelle 400 to 500 feet, Pfaeffers 2128 feet, Gastein 3100 feet, Leuk 4400 feet, Brennersbad in Tyrol (72°.5), 4500 feet; the warm springs of Dux (56°.75 to 72°.5, discovered by Ennemoser), 5600 feet. In the Cordilleras rise the sulphurous springs of Juan (89°.6) and Aqua Tibia (96°.8), at a height of 12313 feet above the sea. Boussingault remarks (Annales de Chimie et de Physique, vol. lii. p. 181), that at different points in the Cordilleras, there are appearances which speak in favour of the opinion that the heat of warm springs is caused by the superior temperature of the interior of the earth. Thus, it seems that the hot springs in the chain of Venezuela have a less elevated temperature the higher they are situated; for example, the hot spring at Las Trincheras, near Puerto Cabello, almost on the level of the sea, has 206°.6; the spring of Mariana, 1465 feet above the sea, only 147°.2; and the waters of Onoto, at a height of 2161 feet, only 112°.1. In the trachytic districts, particularly in the vicinity of volcanos, this regularity in the decrease of the temperature of the springs is no longer observable; and it seems that in this case the local cause, which occasions the volcanic phenomena, has an extraordinary influence upon the temperature of these waters. Anglada (p. 54) shews, on the other hand, that the temperature of thermal springs does not always decrease with the increase of elevation at which they rise. But how could such be always expected, even if they were only indebted to the heat of the interior of the earth for their superior temperature? For, since their temperature depends upon the depth to which the meteoric waters sink through the clefts, it may easily happen that a spring rising at a greater elevation, but coming from a greater depth, should have a higher temperature than another rising at a lower level, but from a lesser depth.

CHAP. II.—*Can the elevated temperature of acidulous springs be a consequence of the absorption of carbonic acid gas?*

Leop. von Buch,† in the communication of his observations on the temperature of the springs in the *Canary Isles*, finds it very remarkable how small a proportion of carbonic acid gas is sufficient to influence the temperature of springs. "But however astonishing," he says, "this circumstance may be, it is nevertheless not peculiar to these islands, but, on the contrary, of rather general occurrence. At least I have not as yet been able to discover any acidulous waters, whose temperature has not invariably exceeded that of the fresh water springs."

The experiments of Henry,‡ by which he found that carbonic acid gas and water of equal temperature, acquired, by their mixture, an increase of temperature of only 0°.45 to 0°.743, were already unfavourable to this hypothesis. However, in order to ascertain what increase of temperature would actually result from the absorption of heated carbonic acid gas by water, I formed carbonic acid gas by heating carbonate of lime in a gun-barrel, and made it pass into a receiver filled with water. The quantity of water in the receiver was 174.5 volumes, and that of unabsorbed carbonic acid gas, which had collected over the water, 60 volumes. The temperature of the water at the beginning of the experiment was 44°.375, and after the absorption of the gas 45°.275; total increase 0°.9.

A part of this increase must, however, be attributed to the heat of the room (54°.5 to 56°.75) during the hour and a half that the experiment lasted, and to the strong charcoal fire necessary for the evolution of the gas, notwithstanding that the receiver was guarded from the radiated heat by a screen.

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* Gilbert's Annalen der Physik, vol. lxii. p. 174.

† Poggendorff's Annal. der Physik, vol. xii. p. 415.

‡ Philosophical Transactions for 1803, p. 1.

To ascertain the temperature of the gas at the moment of its leaving the gun-barrel, I introduced a very delicate thermometer immediately into the stream of gas at the moment of its escape; it rose, however, only to 88°.25, although the gun-barrel, at the end from which the gas issued, had a temperature of 144°.5. A repetition of this experiment gave the same result.*

If, then, we suppose that, at a certain depth in the earth, carbonic acid gas is generated in a similar manner, by the heating of carbonate of lime to a red heat, and that it is absorbed at a certain height above this subterranean laboratory by the waters of springs, the temperature of those springs could only be raised about 0°.9.

Leop. von Buch says farther, that the elevated temperature of acidulous springs is easy to be understood, if we consider for a moment how they make their appearance on the surface. They always owe their existence, namely, to the escape of carbonic acid, from hot mineral springs strongly impregnated with gas, which exist in clefts or in narrow valleys, at a considerable depth. The carbonic acid expelled by the hot water escapes, making a passage up through the cracks in the rocks, combines with the colder waters with which it meets, and comes to the surface in the shape of acidulous springs, bearing a temperature somewhat above the original temperature of the water.

In order to make a trial of this hypothesis by means of a few direct experiments, I evolved carbonic acid gas by boiling an acidulous water, which was very rich in carbonic acid, in a retort, and introduced a very delicate thermometer into the mouth of the retort, immediately in the stream of gas. The temperature of the mineral water before the experiment was 57°.65, that of the air at the mouth of the retort 64°.175. The thermometer did not rise to 65°.75 until some drops of water had distilled over. At this point it remained for several minutes, whilst the mineral water continued to boil, and it was not until aqueous vapours were visible that it rose by degrees as high as 212°.

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* The greater part of the heat which the carbonate of lime receives, seems to be applied in converting the carbonic acid into the gaseous state. It is singular that carbonic acid gas disengaged from chalk by means of concentrated sulphuric acid, acquires a much higher temperature than that which is liberated by heat. The temperature of the gas disengaged by sulphuric acid rose to 133°.25, and would, doubtless, have risen still higher, had not the mass, in consequence of the too violent escape of the gas, come into contact with the thermometer. The temperature of the mass itself was much above 212°.

It is, therefore, quite evident that the carbonic acid gas had a very small share in the raising of the thermometer, but rather that the aqueous vapours produced this effect.

I repeated this experiment, and luted a receiver filled with distilled water air-tight into the neck of the retort. The volume of the mineral water in the retort was to that of the distilled water in the receiver as 1 to 0.77. As the mineral water was over-saturated with carbonic acid, the water in the receiver could still, after subtracting the quantity of carbonic acid left in the mineral water, absorb nearly sufficient to saturate itself. The acidulous water was heated by degrees to 187°.25,* and kept for a long time at that temperature. The total increase of temperature, however, only amounted to 0°.675; which is just the same as was observed by Henry.

I again varied the foregoing experiment by causing a stream of carbonic acid gas, which I evolved from an acidulous water by boiling, to pass through a column of water 18 inches high. The volume of the column of water was equal to 1, that of the water from which the gas was evolved equal to 3.37, that of the space in the retort unoccupied by the mineral water equal to 6.63, and the volume of the gas contained in the water may be taken at least as equal to 4. As the boiling was kept up so long as gas continued to rise, about 10.63 volumes of carbonic acid gas and atmospheric air must have passed through the column of water. The temperature of the air during the experiment was 60°.125, that of the column of water at the commencement 54°.5; and, until the water in the retort began to boil, the highest temperature it acquired was 61°.8. But afterwards, when great quantities of aqueous vapour accompanied the carbonic acid gas, it rose by degrees to 180°.5; and it is evident that these vapours must have also had some share in the increase of 7°.2 observed before the mineral water began to boil.

* The cement with which the thermometer was luted into the tubulus of the retort would not allow of a stronger heat.

From all these experiments it follows, that the heat of acidulous springs cannot be ascribed to the carbonic acid gas itself, but principally to the aqueous vapours which accompany it, for, as Von Buch imagines the production of acidulous springs, the vapours of water must also assist in raising their temperature. Besides, this hypothesis presupposes the existence of an elevated temperature below the origin of these springs; so that we need only suppose them to descend into the vicinity of those hot springs, and they will acquire an elevation of temperature, independently of the inconsiderable increase caused by the absorption of the carbonic acid gas.

If the rising of springs follows the laws of hydrostatics, it is easy to conceive how carbonic acid gas and aqueous vapours should find their way into the channels through which they flow. I have made several experiments on this subject.* I connected two glass-tubes, each of four feet long, by a brass pipe, in such a manner as to form an inverted syphon. At the side of the brass tube, another tube was fixed at right angles to it, having a cock in it, the opening of which was very narrow; to this was luted a small tubulated retort. When the cock was shut the apparatus formed an uninterrupted syphon; and even when it was open, the water continued to flow out of the shorter tube, so long as it continued to be poured into the longer one; for the air in the retort was only compressed, without its being able, on account of the smallness of the aperture, to escape through the cock, and let in the water in its place. When carbonic acid gas was generated in the retort, and the cock opened, the gas rose through the water in the shorter tube in separate bubbles, and escaped from the water collected in a small basin fixed on the top of that tube. During the evolution of the gas in this apparatus, which is a true representation of the course of a mineral spring, the water flowed without interruption out of the basin. Now, as, under these circumstances, where each bubble of gas entirely filled up the channel, and, at the moment of its escape, caused an interruption in the course of the water, there was no perceptible interruption in the flowing off, still less can such an interruption take place in nature, where the bubbles must certainly very seldom fill up the channels through which they pass.

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* Poggendorff's Annal. vol. xxxii. p. 261.

If, lastly, we suppose the carbonic acid gas to be forced into the course of the water under a great hydrostatic pressure, it must be immediately absorbed, and must therefore have still less power to disturb, and will only disengage itself again by degrees in higher regions as the hydrostatic pressure gradually decreases.

Thus we see that the possibility of the creation of acidulous springs, according to Von Buch's hypothesis, is by no means to be denied. Whether all such springs originate in this manner may, however, be a matter of doubt. By far the greater part only exceed the mean temperature of the neighbouring fresh-water springs by one or a few degrees. The origin of these cannot, therefore, be very deep; and yet the aqueous vapours, which must necessarily join them in their course, in order to effect an increase in their temperature, since that caused by carbonic acid gas alone is scarcely perceptible, must come from a great depth. In places where volcanic action still exists, the appearance of fumaroles (evolutions of aqueous vapour) on the surface of the earth is very frequent; as, for instance, Italy (viz. in Tuscany), in the Lipari Islands, and so on. But is any thing similar to the fumaroles to be met with in countries of extinct volcanic action, in which most of the acidulous waters are found, such as the neighbourhood of the Laacher See, the volcanic Eifel, Bohemia, &c.?

It might be objected, in order to ascribe the heat of acidulous springs to carbonic acid *alone*, that the results of my experiments, made under the ordinary atmospheric pressure, can be no criterion of the increase of temperature acquired by water in the interior of the earth, where a considerable hydrostatic pressure augments the absorption of the carbonic acid gas in a high degree. It is true the increase of temperature of the water will be greater the more carbonic acid gas it absorbs. But when water, having absorbed five times its volume of carbonic acid gas in the interior of the earth, comes to the surface, it can at the utmost retain but 1⅔ its volume of free and half carbonic gas; at least, that is the maximum I have found by many analyses of the richest carbonated springs. The heat which the water has acquired by the absorption of the carbonic acid, independently of the heat of the acid itself, must, therefore, re-escape as the spring rises to the surface, when the gas disengages itself again, and, as is so often the case with rich carbonated springs, issues forth in uninterrupted streams.

The evolution of carbonic acid gas from acidulous springs, which is in some cases so enormous, might perhaps lead us to conjecture, that it for the most part proceeds from streams of gas, which come up from the interior without having yet been absorbed by water. But I have found, by measuring the quantity of carbonic acid gas and water yielded in a certain time by one of the richest carbonated springs, that the gas evolved, and that which was absorbed by the water, together, only made up 5.3 times the volume of the water.*

Supposing, then, that this gas came in contact with the water at a depth of only 170 feet, the hydrostatic pressure which would be there exerted, would be sufficient to cause the absorption of 5.3 times the volume of the water. But this spring, bearing a temperature of 9° above the mean temperature of the place, certainly rises from a much greater depth. The supposition, therefore, that a great quantity of heated carbonic acid gas could assist in the elevation of the temperature of a spring, by passing through its channels, is proved to be unfounded.

It appears from observations made on the Mofettas, which usually succeed great eruptions of Vesuvius, that carbonic acid gas evolved, according to Von Buch's hypothesis, cannot possibly have a temperature equal to that of carbonic acid gas disengaged from chalk submitted to a red-heat in a gun barrel. Thus Monticelli and Covelli† found, that cavities filled with the mofettas, were only 3°.85 warmer than those in which no mofettas existed. If, then, streams of carbonic acid gas, issuing forth so near their volcanic origin as these do, shew so low a degree of heat, it cannot be expected that such as rise in the neighbourhood of extinct volcanic action should be warmer. Nor have I observed a superior temperature in any which I have examined of the numerous exhalations of carbonic acid gas which occur in the vicinity of the Laacher See and in the Eifel. I do not, however, consider this as a sufficient proof, as, in my opinion, all these exhalations proceed from mineral springs situated below them.*

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* Poggend. Ann. vol. xxxii. p. 251.

† Storia de Fenomeni del Vesuvio, avvenuti negli anni 1821, 1822, e parte del 1823, con observazioni e sperimenti di T. Monticelli e N. Covelli. Napoli, 1823. Translated into German by Nöggerath and Pauls. Elberfeld, 1824, p. 193.

If we assume that, from whatsoever cause, water in the interior of the earth is heated to 212°, and that carbonic acid gas of a temperature much above the boiling point be forced at the same time through it; that water would not only not become more heated, but, on the contrary, would suffer a diminution of its temperature, on account of the evaporation. This may, perhaps, be one of the causes that so few of the known hot springs reach the boiling point.

I support this assertion upon several experiments which I made for that purpose. Thus, on driving air heated to the melting point of tin in such considerable quantities as a large double action bellows was capable of giving, for a whole hour, through about 12 ounces of water of 60°.125, it only caused the temperature of the water to rise to 119°.75. Indeed, when the water was surrounded by other water kept continually boiling, the passing of the heated air through it always caused a depression, greater or less, of its temperature; in one case from 194° to 158°. The boiling of the surrounding water decreased in violence perceptibly when the heated air was let in to the water, and, on the other hand, increased, when the stream of hot air was interrupted.

To all the objections already taken to the hypothesis that carbonic acid gas is the cause of the warming of acidulous springs, it may be added, that their temperature does not always exceed that of the fresh-water springs. In April 1833 I found the temperature of the acidulous waters of Meinberg, and of the carbonic acid which is disengaged from them in great quantities and with great violence, to be only 41°.45. Among the observations communicated in Chap. VI. on the temperature of these springs at different seasons, we find that their yearly mean differs but little from that of the neighbouring fresh-water springs. The carbonic acid cannot, therefore, increase their temperature at all, although they contain 0,9 and 1,31 times their volume of that gas.* Every thing speaks in favour of the supposition, that these acidulous waters are formed near the surface, where streams of water and carbonic acid gas come in contact with each other.

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* Poggend. Ann. vol. xxxii. p. 248.

Lastly, if we take into consideration that by far the greater part of the fresh-water springs of artesian wells, which seldom contain a larger proportion of carbonic acid than those of common wells, have, however, a temperature exceeding the mean temperature of the place in many cases by several degrees, we find ourselves obliged to seek another cause for the elevated temperature of such springs. I only bring forward, as an example, the above-mentioned warm-springs of Paderborn, whose temperature is as high as 61°.25, and which, however, only contain one-tenth of their volume of carbonic acid gas, whilst the neighbouring springs of Schmechten, Hester, Driburg, and Pyrmont, so rich in carbonic acid, only give 48°.2 to 54°.5. In like manner, the temperature of the salt-springs of Westphalia falls between 52°.25 and 63°.5, although they are also very poor in carbonic acid.

CHAP. III.—*Can the Heat of Thermal Springs be the result of chemical processes or of local circumstances? and can local circumstances cause any modifications in the Temperature of thermal springs?*

With regard to the former of these questions, two cases may be imagined: either that the heat of the springs is the result of chemical processes acting in the mineral waters themselves, during their formation, and by which their mineral contents are produced, or, that chemical processes take place in the vicinity of the course of the mineral springs, by which their channels become heated from without.

Respecting the first case, it is quite evident, that such a chemical process cannot be the solution of salts already in existence; for then, on the contrary, (with one exception only, namely, when anhydrous salts, which require much water of crystallisation, come in contact with water), cold would be the result. But since salt-water springs are also thermal, and, indeed, warmer the more salt they contain (which is considered a general rule by salt-workers), and as it may be considered quite certain that they derive their saltness from deposits of rock-salt, a contradiction here presents itself, and we must consequently conclude, at least for salt-water springs, that they are not indebted to chemical processes of the first kind for their superior temperature.

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* The salt-spring at Schieder, on the other hand, though only three miles from Meinberg, and containing only 0,37 volumes of carbonic acid gas, is, nevertheless, warmer than those carbonated springs.

The greatest evolution of heat would take place, if the elements of the salts, contained in the mineral springs, entered into combination at the moment of the creation of those springs. In order to have an idea of the degree of heat which would be produced in this case, I made the following experiments:—

The Heilbronn, a mineral spring in a small valley of the Brohl, four miles distant from the lake of Laach, is, next to Bilin in Bohemia, the richest in carbonate of soda known to me. It contains 0,0053 of fixed substances. Supposing that this spring were formed from anhydrous carbonate of soda, by the addition of concentrated sulphuric acid, muriatic acid, and water, then, according to my analysis, 77,4 parts anhydrous carbonate of soda, 5 parts concentrated sulphuric acid, 92 of smoking muriatic acid, and 22,687 parts of water would be required to compose a water, containing the same proportions of carbonate and sulphate of soda, and of chloride of sodium, as that spring. In accordance with this, therefore, I put 77,4 grains of calcined carbonate of soda to 22,687 grains of water. The temperature of the water was,—

Before the experiment, ...	42°.8
After, ...	43°.7
Increase of temperature,	0°.9

To this solution of soda I added a mixture of 5 grains concentrated sulphuric acid, and 92 grains smoking muriatic acid. The temperature of the two liquids was,—

Before their mixture, ...	50°.00
After, ...	50°.45
Increase of temperature,	0°.45

Now, although such a chemical process as this, which is very improbable to take place in the interior of the earth, is the most [illegible] for the production of heat; still, it only caused an increase of temperature of 1°.35. Since, then, atmospheric water, under similar circumstances, having the temperature of the place, would, by dissolving so much of the above-mentioned substances as to form one of the richest mineral springs of Germany, only acquire a temperature of 1.°35 higher than that of the neighbouring fresh water springs, the increase of temperature caused by this process in such mineral springs as frequently only contain one-half to one-sixth as much of soluble ingredients as the Heilbronn, could not be perceptible.

If we even admit the most favourable case for an increase of temperature, but *in rerum naturâ* the most improbable case of all, viz. that sodium should come in contact with water containing the necessary quantities of sulphuric, muriatic, and carbonic acid to form such a mineral water as the Heilbronn; even then, only an inconsiderable elevation of temperature would take place, as the following experiments verify.

Fifteen grains of sodium were put into a goblet, and a small glass funnel placed over it; upon this were poured 1000 grains of water in which so much sulphuric and muriatic acid were mixed as would form such a mineral water as that of the Heilbronn. A violent explosion, accompanied with a shower of fire, was the consequence, and the glass broke.

The heat of the water was,—

Before the experiment, ...	52°.925
After, ...	55°.850
Increase of temperature,	2°.925

In a repetition of the experiment in a metallic vessel, the increase of temperature was 4.275.

The increase of temperature, caused by the oxidation of the alkaline metals by water, is, therefore, far from sufficient to account, as Von Buch has attempted,* for the existence of hot springs. Neither is there more probability in Boussingault's hypothesis,† that the hot-springs which rise in the granite of the Littoral Corderillas may be the result of the action of water upon sulphuret of silicium, by which warm water, containing silica and sulphuretted hydrogen, would be produced; for these springs contain very little of these substances.

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* Abhandlungen der K. Akad. der Wissenschaften in Berlin aus den Jahren, 1818, 1819, p. 65.

† Annal. de Chimie et de Phys. Février, 1833, p. 189.

The formation of mineral springs by the decomposition of water by the alkaline metals, is, indeed, contradicted by the circumstance that, in that case, streams of hydrogen gas must issue with the water, which has not as yet been observed in any springs.

There is also another argument against the possibility of any chemical process being the cause of the heat of thermal springs, which is in some cases so very considerable, namely, that it is by no means a general rule that those springs, which contain the greatest quantity of fixed substances, are the hottest. Thus, for example, within the space of about one mile, in the vicinity of the Laacher See, are found the following mineral springs, whose quantities of fixed constituents and temperatures confirm my assertion, at least for that neighbourhood.

	Fixed Constituents.	Temperatures.
Heilbronn, . . , .	0,0053	52°.925
Tönnistein, ...	0,0025	54°.950
Fehlenbor, ...	0,0019	57°.650
Burgbrohl, I. . . .	0,0013	58°.100
Burgbrohl, II. . . .	0,0008	53°.600

Still less is it the case with the alpine springs Gastein and Pfäffers, which contain less fixed and gaseous substances than common spring water, and are consequently little else than pure warm water; not to mention the pure water springs and artesian wells, which are frequently warmer than neighbouring mineral springs containing fixed substances in great abundance.

If there existed mineral springs consisting of a concentrated solution of sulphate of iron, a superior temperature would be more conceivable; although even then the elevation could hardly be perceptible, as the slow decomposition of the magnetic pyrites in the copperas manufactories proves.

Against the second case, namely, that chemical processes take place in the vicinity of the course of mineral waters, by which the sides of the hollows and clefts through which they flow become heated from without, several objections may be made. In the first place, traces of such chemical processes would surely be visible, if the seat of their action were in communication, by means of the clefts, with the surface. But in that case, the meteoric waters would also have free access to that place, and would either increase, diminish, or entirely extinguish the chemical action; or would somewhere reappear charged with substances which had taken a part in the process. Of this, however, experience furnishes no examples. Secondly, if we suppose the process to be an oxidation, the free admission of the oxygen of the air would be necessary. But then the atmospheric waters would also have access, since such a process cannot be conceived without a communication with the atmosphere, and nitrogen gas would be evolved in much greater quantities than are found here and there sparingly emitted from some few mineral springs.

Thirdly, if this oxidation be supposed to take place at the expense of water, hydrogen gas must be evolved, which is also contrary to experience. Such an explanation of the heat of mineral springs necessarily presupposes a structure in the interior of the earth of quite a peculiar nature, viz., one cavity inclosed within another, and one to which nothing analogous has been found in the working of mines; the theory must, therefore, be considered as untenable.

It must, however, not remain unmentioned, that subterraneous fire may in solitary instances give rise to warm springs. Examples of this have occurred in the Planitzer Adit, near Zwickau, and at Holdenstaedt near Eisleben in Thuringia.*

Lastly, it must also be taken into consideration, that formerly, when only the few commonly called hot-springs, such as Carlsbad, Aachen, &c. engaged the attention of philosophers, the explanation of this phenomenon could only be sought in local causes. Becher's hypotheses with respect to Carlsbad, that water containing common salt flows over a depot of burning iron-pyrites, or Klaproth's supposition that the water is heated by a considerable bed of coal, set on fire by iron-pyrites, were, therefore, for the moment considered satisfactory. Independently of the well-grounded objections of Von Buch and Berzelius to these hypothesis, there would still remain much for us to account for, even if we had very satisfactory explanations of those properly called hot-springs. For as the rising of thermal springs is a phenomenon of very general occurrence, which may be traced through all formations, from the very youngest of the stratified to the very oldest rocks, the chemical processes which cause the heat of these waters must be equally universal. Or is it, perhaps, less difficult to account for the heat of a spring whose temperature only exceeds the mean temperature of the place by a few degrees, than to find an explanation for the creation of those commonly called hot springs?

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* Kühn Handbuch der Geognosie, vol. i. 279

Anglada[*] adds another argument of no small weight against the explanation of the heat of thermal springs by any chemical process, namely, that it would be difficult to conceive how such a process should continue in action for so long a time, and with such uniformity as would be necessary to explain the uninterrupted course of the springs, their uniform productiveness, and their unvarying temperature and composition. . . .

Since, then, we are obliged to admit that the conditions necessary for the production of warm springs must exist in all parts of the earth, we find ourselves imperceptibly led to the hypothesis of a superior temperature in the interior of the earth.

Laplace already endeavoured to account for the heat of thermal springs, and their uniformity within the memory of man, by this internal heat of the earth.[†] Subsequently, it has been shewn by Arago,[‡] that the temperature of artesian wells is higher the deeper they spring; and he has brought this forward as a proof, that the temperature of the strata of the earth increases constantly with the depth. Anglada,[§] who is also led to this hypothesis, thinks, that if the heat of mineral waters were only caused by the increase of temperature towards the centre of the earth, it would be contradicted by our not meeting every where with the effect of a cause supposed everywhere to exist; as thermal springs are in some places very abundant, whilst in others they are totally wanting.

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[*] Mémoire pour servir à l'Histoire, &c. vol. i. p. 15.

[†] Annal. de Chim. et de Phys. vol. xiii. p. 415.—This philosopher says, "Si l'on conçoit, que les eaux pluviales, en pénétrant dans l'interieur d'un plateau élevé, rencontrent, dans leur mouvement, une cavité de trois mille mètres de profondeur, elles la rempliront d'abord; ensuite, acquérant à cette profondeur, une chaleur de 100° au moins, et devenues, par là, plus légères, elles s'élèveront, et seront remplacées par les eaux supérieures, en sorte, qu'il s'établira deux courans d'eau, l'un montant, l'autre descendant, perpetuellement entretenus par la chaleur intérieure de la terre. Ces eaux en sortant 'e la partie inférieure du plateau auront évidemment une chaleur bien supéeure à celle de l'air au point de leur sortie."

[‡] Annal. de Chim. et de Phys. vol. xxix. p. 317.

[§] P. 17.

It is evident that Anglada only took those commonly called hot springs into consideration. But since thermal springs are so universally distributed over the earth, that which he opposes to our hypothesis, argues, on the contrary, in its favour.

Perhaps, he says, the phenomenon of thermal springs may be best accounted for by the action of electromotors (electro-moteurs), existing in the interior of the earth. Many German philosophers are also known to have inclined formerly to this hypothesis; but the greater part, at least those whose object is not merely speculation but research, have now abandoned it.* It may, however, perhaps, not be quite superfluous briefly to examine the grounds which Anglada, a philosopher to whom we are indebted for so many and such profound researches on mineral waters, brings forward in support of such an hypothesis.

According to him, there are many appearances on the surface of the earth which indicate the existence of electromoters. As, for instance, the Heideberg in the Fichtel-Gebirge, which Von Humboldt discovered in the year 1796 to be polar magnetic.† But by more recent inquiries, I have endeavoured to shew that that polarity is only a consequence of the magnetic pyrites contained in the rock.‡ If, as Anglada supposes, there exist an electromotric power corresponding with the magnetic polarity, and if the former is the cause of the heat of thermal waters, then springs rising in the vicinity of lodes or beds of magnetic iron-ore (oxydulated iron) must always have an elevated temperature. But this is by no means the case; for example, in Sweden, Siegen, and the Duchy of Westphalia, where considerable lodes and beds of that mineral are found.

Anglada is of opinion, that although the constant running and the uniform temperature of warm springs agree very well with the opinion that they owe their heat to that of the interior of the earth, it would still be impossible to account by that means for the accidental variations which the temperatures of springs suffer. And he maintains that his hypothesis is satisfactory in this respect, the more particularly as such alterations generally take place during earthquakes, and therefore prove themselves dependent upon those great electrical phenomena (?).

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* See Bischof, die Vulkanischen Mineralquellen, &c. p. 321 and following. Perhaps I have the merit of having assisted in the downfall of this singular hypothesis.

† See Gren's Neues Journ. vol. iv. part 1, p. 136; and Von Humboldt's subsequent remarks in the Annal. de Chim. et de Phys. vol. xxv. p. 327.

‡ Goldfuss and Bischof Physikal-Statische Beschreibung des Fichtelgebirges, 1817, part 1, p. 193, and following.

These objections are easily set aside. If alterations of the temperature and chemical composition of springs generally happen during earthquakes, they may be caused by the opening or closing of fissures, by which the waters come into contact with other substances, or, by their coming from a greater depth, bring with them the loose contents of the clefts in the rocks. If the fissures open further, the atmospheric waters will sink deeper, and become more heated; if they close, the contrary will take place. According to Dr Ambrozzi,* the warm waters of Teplitz flow more copiously since the earthquake which destroyed Lisbon on the 1st November 1755, than before; during the earthquake they became muddy, flowed one hour and a half of a dark yellow colour, and towards mid-day entirely disappeared during six or seven minutes, then suddenly gushed forth again, and for half an hour continued to emit a thick yellowish-red water, in such quantities that the baths were overflowed; whereas the medicinal springs of the village of Schönau, of Carlsbad, and all others, with the exception of some in Morocco, remained unaltered.

An alteration of temperature and chemical composition has often been observed in springs which rise in the vicinity of active volcanos. Thus, according to Dolomieu,† the spring of Macaluba, which in the year 1781 emitted atmospheric air and carbonic acid gas, only yielded a kind of inflammable gas in the year 1785. Other examples of alterations taking place in the temperature of springs situated near active volcanoes will be given afterwards. On the other hand, springs rising in the neighbourhood of extinct volcanos shew a great uniformity of temperature and chemical composition during long periods of time.

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* Physikalisch-Chemische Untersuchung der Warmen Mineralquellen zu und bey Teplitz. Leipzig. 1797.

† Sur les Isles Ponces, p. 368.

The same effects which earthquakes are capable of causing in springs, will also be produced by active volcanos, for volcanic action is always accompanied by earthquakes. Besides, by the rising of the melting lava, the upper strata of the earth become heated, and thus it may happen that springs rising in a volcanic soil, although from an inconsiderable depth, may, as Anglada himself remarks, be warmer than others which do not rise in volcanic districts.

All such variations may, therefore, take place without injury to the hypothesis, that the superior temperature of the interior of the earth, progressively increasing with the depth, is the principal cause of the heat of thermal springs. It is, therefore, unnecessary to seek refuge with Anglada in such an improbable subterranean electrical process.

It still remains to be ascertained whether a gradual alteration of the temperature of springs, in places where there are neither traces of former nor of present volcanic action, and where earthquakes are not frequently felt, can be conceived possible. With respect to the phenomenon itself, it is difficult to determine whether it exists or not; for all depends upon the thermometers, which, so constructed as to serve for a comparison of observations on the temperature of springs, have only been in existence for about one century. But it is well known that even until a much later date, exactly corresponding thermometers were considered great rarities; and it is not long since attention has been drawn to several circumstances, upon which the accuracy of these instruments depends. It is true that considerable differences of temperature may be observed in springs even with inaccurate thermometers; but if they proceed by degrees, and are not perceptible until a long period has elapsed, the most perfect apparatus is necessary for their detection. According to the observations of Carrere in 1754, and those of Anglada in 1818 and 1819,* the temperature of ten thermal springs in the East Pyrenees, has decreased 1°.125 to 60°.75 during that period, and the decrease was found to be greater the higher the original temperature of the springs.

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* Anglada, p. 63.

Besides the accidental causes which may lower the temperature of thermal springs, such, for instance, as meeting with colder waters, there seems to be another very important one, viz. that mineral springs frequently fill up their own course by a partial precipitation of their constituents. If such a precipitation takes place in the lowest part of the spring's course, and advances gradually higher and higher, the consequence naturally must be, that the meteoric waters, no longer able to sink down to so great a depth, by degrees become less and less heated. The deposit from the Carlsbad springs may here be cited as an example; although in this case the deposit is only observed on the surface, and the cause of its precipitation above cannot so easily exist below. The enormous deposits of travertin are well known. In that occasioned by the hot springs of San Filippo, fissures of 30 feet deep and 150 to 200 feet long are observed. A particularly important and direct proof is the calcareous deposit in the Roman aqueduct between Cologne and Treves. It has been deposited since the time of the Romans, from several fresh water springs, and is seven or eight inches thick in some places, so that it has been used for columns of churches.

I have elsewhere [*] called attention to the veins of brown ironstone in the trass of the Brohlthal, which may often be traced to a great distance on the bare walls of ravines and quarries in that rock, and which certainly owe their existence to no other cause, than that ferruginous waters, of which there are still a great many in existence in that district, formerly flowed in those cracks, and by degrees deposited hydrate of iron, thereby filling up their own course. Such fillings up with hydrate of iron are also found in the greywacke, in basalt, in trachyte, [†] and other rocks. Inferring from the deposits of spherosiderite from mineral springs actually observed, I have endeavoured to shew that veins of carbonate of iron and brown ironstone may also often be the sediment of mineral springs.

I have already proved [‡] that when springs contain any sulphates, together with iron and an organic substance, iron-pyrites is very easily formed, and in this manner they may fill up their subterranean channel. This may also happen when two different mineral springs meet in one cleft, one of which contains carbonate of protoxide of iron, and the other a sulphate and an organic substance. And nothing is more common in mineral springs than these substances. In general, many cases may be imagined, in which the confluence of mineral springs composed of different ingredients might occasion such sediments as would by degrees fill up their passage. For example, if springs containing earthy salts should meet with others containing alkaline carbonates; or if waters rich in silica * should meet with organic substances, whether in solution, or in the solid state, by which, as I have shewn, siliceous concretions would be formed.

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[*] Schweigger-Seidels n. Jahb. der Chemie und Phys. vol. viii. p. 431.

[†] Vues et coupes des principaux formations géologiques du Dép. du Puy-de-Dôme, &c., par Lecoq et Bouillet, Clermont Ferrand, 1830, 8me. livraison, p. 223.

[‡] A. O. vol. iv. p. 386. See also Longchamp in the Annals de Chim. et de Phys. vol. xxxii. p. 294.

In the above mentioned sulphurous springs of the East Pyrenees, iron-pyrites and earthy carbonates might easily be formed at the expense of their sulphuret of sodium and thair alkaline carbonates, by their meeting with other springs containing salts of iron and earths, and thus the channels of these springs becoming gradually filled from the bottom, the waters would by degrees be enabled to sink less and less deep into the earth. †

Lastly, an obstruction in the course of the springs may be occasioned partly by chemical and partly by mechanical means. I have pointed out ‡ several appearances which make it seem very probable, that ferruginous waters act as a cement upon loose stony materials, namely, upon sand, and may thus give rise to the formation of stony concretions. There is no doubt that waters containing much silica occasion siliceous concretions, such as are found very characteristic in the sandstone belonging to the lignite (braunkohl) formation, and also in other positions. And surely concretions of this kind may also easily be formed in the

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* In the great Geyser, the deposits of silica have accumulated in a crack to the thickness of 12 feet.

† Supposing the increase of temperature in plains and in enclosed valleys to amount to 2°¼ in 115 feet, it is easy to perceive, that if the lower parts of a spring's course be very narrow, and the spring be rich in any substance easy to be precipitated from it, no great length of time will be required to effect a perceptible decrease of temperature.

‡ Schwegger-Seidels n. Jahrb. der Chemie & Phys. vol. viii. p. 437.

channels of springs from sand and stones mechanically torn off or borne along by the stream. Since hot springs and aqueous vapour are able to act very powerfully even upon hard rocks, such, for instance, as marble, and thus to acquire quite a muddy consistence, as I have observed in the Kaiserbad at Aix-la-Chapelle, and in the baths at Burtscheid; the channels of springs may also in this manner become stopped up, especially if the spring have a cementing property.

Anglada supposes that the loss of heat, occasioned in the inner strata of the earth, by warm springs, is not restored by the power of conducting heat of the materials composing the interior of the earth; and that the strata must consequently suffer a gradual diminution of temperature throughout the sphere of action of those cooling agents.

A gradual diminution of the internal temperature of the earth, caused by the loss of the heat carried off by thermal springs, cannot be doubted, so long as that loss is not repaired by any means. But whether that diminution has become perceptible within historical times or not, is another question.

The various degrees of temperature with which atmospheric waters sink into the earth at different seasons of the year, are already equalized in the uppermost strata of the earth's crust; for springs which rise from a moderate depth shew but a trifling variation of temperature throughout the year, and that of thermal waters is, in general, quite constant. The heat carried by the pluvial waters into the earth, is, therefore, lost in the almost inexhaustible provision of the earth's internal heat. The waters having soaked through the earth's exterior crust reach the strata, where the increase of temperature begins, with a constant or nearly constant temperature. At this limit the difference between the temperature of the channels and that of the waters can therefore only be infinitely small; and as the waters, by sinking through strata always increasing in temperature, become gradually warmer, it may be assumed that the difference is infinitely small in every point of their course. That difference can only be perceptible when the waters do not filter through the strata finely divided, but flow in considerable streams. But the smaller the difference between the temperature of the waters and that of the channels through which they flow, the less considerable will be the loss occasioned by the waters in the temperature of the strata. But, even supposing that by degrees a considerable local depression of temperature should take place in the channels of the springs, or, in other words, that in them the increase of temperature towards the centre of the earth should have become less rapid; then the heat in their environs would be conveyed to them the more quickly, because the rapidity with which heat is transmitted from one body to another, increases in proportion to the difference between the temperatures of the two bodies. In no case, then, is it conceivable that a local cooling of the earth should be continually in progress, as Anglada is inclined to assume; so that a gradual diminution of the heat of thermal springs can only be imagined in the case of a general cooling of the interior of the earth. But since the existence of thermometers, and since observations have been made on the temperature of springs, this general cooling has certainly not taken place in a perceptible degree. An actual diminution in the temperature of thermal springs, if no longer doubted, can therefore only proceed from the causes above enumerated.

CHAP. IV.—*Can Springs convey Heat from the Interior of the Earth to the Surface?*

The universal occurrence of warm springs is alone sufficient to answer this question. But, independently of that, the possibility of springs pursuing a very long subterranean course, wherever their temperature may have been obtained, without suffering any considerable change, may also be indirectly shewn.

The coldest springs of the temperate zones rise in the vicinity of the glaciers, and on the limits of perpetual snow. Professor Ennemoser, at my request, determined the temperature of thirteen fresh-water springs near the glaciers, and on the limits of the snowy regions of the Tyrolese Alps, in the summer of 1833, and found them to vary from 36°.50 to 43°.2.

On the 28th August 1835, I found the temperature of four fresh-water springs, at the foot of the Gandecke, or Morene, of the upper glacier near Grindelwald, in Switzerland, 3684 feet above the level of the sea, to fall between 37°.40 and 38°.075; and, on the 3d of September, I found the temperature of fifty-one fresh-water springs on the Spital-Matte, between Kanderstäg and the Gemmi, 5887 feet above the sea, from 37°.85 to 40°.10.

According to former observations of Wahlenberg and Von Buch,[*] the springs on St Gothard, 8587 feet above the sea, have a temperature of 37°.4; and, according to Stampfer and Thurwieser[†], one of the last springs, surrounded with vegetation, on the way to the Grossglockner, 6660 feet above the sea, in the vicinity of the glaciers, has a temperature of 38°.075. Thus we find the lowest temperature till now observed, near the limits of perpetual snow, to be in summer 36°.5.

There is no doubt that many springs sink down from the snowy regions, through fissures in the rocks, and do not return to the surface until they have reached much lower levels; and since they are capable of maintaining their low temperature, notwithstanding the higher temperature of the strata through which they have passed, it is evident that springs must be met with far below the limits of perpetual snow, bearing a temperature nearly the same as that of the springs rising near those limits.

In the Etschthal, above Partschins, near Meran, about 3000 feet above the sea, on the steep southern declivity of the mountain, Ennemoser found a very considerable spring rising in three places (the Oberhauser springs) to have a temperature of 41°. According to the observations of an apothecary in Meran, this temperature is constant throughout the year. The springs in the Etschthal itself, 1000 to 1200 feet above the sea, he found to have 50° to 54½°. It certainly cannot be supposed that, 1800 to 2000 feet above a country where figs and other southern fruits ripen, the temperature of the soil should be only 41°, nor that such a considerable difference of temperature could be attributed to so small a difference of elevation; but it seems rather that the spring near Meran derives its low temperature from its having descended from the neighbouring mountains.[‡] This is the more probable, as other springs in the neighbourhood of this one, and at an equal elevation, have 45°.5 to 47°.75.

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[*] D'Aubuisson, Traité de Géognosie, vol. i. p. 427.

[†] Jahrbücher des K. K. Polytechnisen Instituts in Wien. vol. vii. p. 2.

[‡] See Kupffer in Poggendorff's Annalen. vol. xv. p. 165.

Now supposing that this spring originally had the temperature of springs on the limits of perpetual snow, it would, after a subterraneous course of 5200 feet, only have suffered an increase of temperature of about 3°.15.

In Passeyr, 5000 to 6000 feet above the sea, the fresh-water springs, according to Ennemoser's various observations, have a temperature of 41° to 43¼°. However, he found great variations here also. For instance, near Hittermühle, at an elevation of 5000 feet, rises a copious spring of 40°.1, whilst others 600 to 800 feet higher gave 42°.125 to 45°.5. At Hitte, behind Platte, there is a celebrated spring at an elevation of 4000 feet, which shews 41°, and the Goldbrunnen only 38°.75, whilst other neighbouring springs shew 43¼° to 47¾°. On mountains of 7000 and 8000 feet, the springs have a temperature of 38¾° to 41°, with but few exceptions.

In several places in Switzerland I found springs, the temperatures of which were also much lower than might have been expected from the surrounding vegetation. Thus the temperature of four springs at the foot of the Great Eiger, near Grindelwald, which rises almost 9000 feet perpendicularly, was only 42¾°.

Von Buch* found a spring near Neufchatel, in the Creux-du-vent, 2073 feet above the Lake of Neufchatel, and 3337 feet above the level of the sea, the temperature of which was 40°.437, whilst others nearly on the level of the lake, observed in different seasons and under different circumstances, gave 50° to 50°.45. He also found† that the temperature of the springs on Teneriffe shewed no great variations up to 4000 feet above the sea, and that, in like manner, the temperature of the springs on the northern declivity of the Gran Canaria is 62°.375, up to 2000 feet above the sea. This can certainly not be accounted for in any other way than by supposing that they derive their low temperature from high mountains from which they had descended, and that, in their quick subterranean course, they have preserved an equal temperature at heights differing often by several thousand feet.

Dalton‡ observed the temperature of a copious spring on

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* Gilbert's Annalen. vol. xxiv. p. 50.

† Poggendorff's Annalen. vol. xii. p. 413.

‡ Meteorological Essays, or Gilbert's Annal. vol. xxiv. p. 50.

Mount Helvellyn, near Kendal, 2700 feet above the sea, and 393 feet below the summit of the mountain, on the 27th August, to be 38°. So low a temperature for a spring rising in such a moderate elevation, in the north of England, is somewhat remarkable.

Von Humboldt* likewise mentions several springs in the mountains of Cumana and Caraccas, whose temperature is much lower than might be expected from their elevation. Hunter's well-known observations on the temperature of springs in Jamaica, afford examples of similar appearances. Humboldt already observed that one of those springs, rising at a height of 3918 feet above the sea, probably derives its very low temperature from the peak which rises to 6966 feet.

That even waters flowing on the surface of the earth, and bearing a temperature very different from that of the atmosphere, change their temperature but very slowly, is particularly evident from the brooks which issue from the glaciers, and which frequently, after a long-continued course, suffer a scarcely perceptible increase of temperature. On the western declivity of the Teutoburger Wald, where such copious springs occur that they immediately form considerable rivers, I also found that one of these rivers, after a course of half a mile, had only become 0°.675 warmer, although the temperature of the air was 22°.5 higher than that of the water.

All these observations shew that springs which descend from great heights bring down cold with them, and indeed the more copious they are, the quicker their subterranean course, the steeper the mountains, or the nearer their channels approach the vertical position, and the less they are adulterated on their way with waters of a different temperature, the greater is the degree of cold which accompanies them. From which it follows, conversely, that springs which rise from below, out of various strata of the earth, will bring heat with them, and in a great degree the more the above conditions are fulfilled.

The more the temperature of springs surpasses that of the strata through which they pass, the more they will lose of their temperature. Boiling springs seem only to reach the surface, where volcanic fire also shews itself, as, for example, in Iceland,[*] on the Lipari Islands,[†] and Las Trincheras near Puerto Cabello, in the Corderillas.[‡] But some springs would certainly reach the surface with a higher temperature, did they not meet on their way with colder ones; and springs running in narrow channels must cool more rapidly than in larger ones.

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* Gilbert's Annal. vol. xxiv. p. 46.

Chap. V.—*The Temperature of Springs being a function of that of the Meteoric Waters, and of the strata of the earth through which they flow, it is required to determine whether the Variations of the Temperature of the Meteoric Waters also shew themselves in Thermal Springs.*

Springs which only pass through those strata which participate in the variations of the external temperature, must themselves have a variable temperature. The table at the end of the next chapter, which contains my own observations on the temperature of springs, as well as all those of others which have come to my knowledge, affords many examples of such variable springs.

As in our latitude the yearly mean temperature of the air occurs about April and October, its maximum in July, and its minimum in January, it will be seen by the Table, that the variations of temperature of some few springs keep an equal pace with those of the air. In most cases they follow one, two, or even three months later.[§] This depends on the depth below the surface in which the course of those springs lie, the quantity of water they yield, and the power of conducting heat possessed by the soil. The deeper springs flow, the less abundant their waters; and the less the degree of conductibility of heat of the soil, the later will the seasons of their temperature follow those of the air. The differences between the annual maximum and minimum temperature of springs are, in general, greater the nearer their variations of temperature coincide with those of the air, and conversely. The Table shews these differences to be greatest, when the maximum temperature of the springs occurs in July or August, and the minimum in January and February.

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[*] Gilbert's Annalen. vol. xliii. p. 54.

[†] Poggendorff's Annal. vol. xxvi. p. 67.

[‡] Annales de Chimie et Phys. vol. lii. p. 181.

[§] In the Journ. de Phys. vol. lxviii. p. 224, it is asserted that the minimum temperature of springs happens at the time of the maximum temperature of the air, and *vice versa;* for a thermometer, which was suspended in a well of 34 feet deep at Geneva from 1796 to 1805, gave a maximum of 54.275 in December 1804, and a minimum of 48°.425 in June 1798.

In the Table we find that the difference between maximum and minimum most frequently does not amount to 2°.25, and that differences exceeding 11°.25 are of rare occurrence. The greatest difference is 26°.25, with the exception of the springs of Stuttgart and Tübingen. As the temperature of these springs was observed at the outlet of long aqueducts running at a very small depth beneath the surface, it is evident that the temperature of the air must have had some effect upon it. So that, in this respect, the differences of temperature of those springs cannot serve for a comparison.

A singular relation is observed to exist with regard to the annual variations of temperature of the salt springs of Werl. They correspond almost exactly with the variations of temperature of the air, and yet the springs are undoubtedly thermal. If these were produced in the same manner as the very copious springs of the Jordan, Lippe, Pader, Heder, and so forth, which rise on the western declivity of the Teutoburger Wald and the Haar, it would be easy to conceive that their variations of temperature should correspond with those of the air; for those springs are, in fact, rivers, which having sunk from higher regions into the numerous clefts in the chalk rocks, pursue a subterraneous course, and reappear at a much lower level. Such considerable bodies of water, in sinking to a depth where the increase of temperature towards the centre of the earth is perceptible, do not entirely lose the temperature which they had acquired whilst running on the surface; so that, notwithstanding their appearance as thermal springs, the variations of the external temperature still shew themselves in them.[*]

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[*] Between the variations of temperature and of the contents of salt of these springs no relation can be found. Their mean temperature in 1833 was higher than in 1832; whilst, on the other hand, the quantity of salt contained in them was much smaller. Besides, the maximum of salt contained in them falls constantly in May, whilst the minimum has no determined period. This circumstance indicates a very peculiar, or, I would say, a complicated combination of the subterranean channels at Werl; as does also the fact, that different borings in that neighbourhood, often situated very near to each other, yield sometimes strong salt-waters, and sometimes springs of fresh-water.

These salt springs prove, therefore, that even thermal springs can participate in the variations of the external temperature. The differences between maximum and minimum are found, however, to diminish, as the mean temperature increases. But for this very reason it is improbable that springs originating in the superficial strata, in which the variations of the external temperature are still felt, should shew a constant degree of heat. Springs of constant temperature must, therefore, always be considered as thermal. The difference between the temperature of the coldest of the constant or nearly constant springs, and the mean temperature of the neighbouring variable springs, is not the same for all places. According to the table, these differences are, for the neighbourhood of Berlin 0°.675; in Sweden hardly 2¼°; near Burgbrohl, about 2¼ miles from Bonn, they rather exceed 4½°, and the warmest of the salt springs of Werl, which surpasses the probable temperature of the coldest of the neighbouring springs by 6°.075, is not yet constant, but shews a yearly variation of temperature of 4½°.*

It may be that the extent of these differences of temperature at a certain place, depends, in some measure, on its geographical latitude, as, according to Chap. VIII, the depth to which the external temperature continues to be felt, is greater in higher than in low latitudes, the annual variations of temperature of the air being also greater there than here. But the less considerable these variations are, the less are also the yearly variations of temperature of the waters which filter into the earth, and the more easily will they, after a short percolation of the earth's crust, give rise to springs of a constant temperature. The influence of latitude upon the scale of the annual variations of temperature must, however, certainly be very much modified by other circumstances,—namely, the elevation above the surface of the sea at which the springs rise, the filtering of the atmospheric waters in more or less considerable streams through the earth, and the degree of conductibility of heat of the soil. The extent of the variations of temperature during the year, diminishes as the height increases; so that in higher latitudes the greatest difference of temperature at a certain elevation is not greater than under the equator. The more finely divided the atmospheric waters are during their infiltration, and the less the degree of conductibility of heat of the soil, the sooner will springs of a constant temperature be formed, as will be shewn more fully in Chap. VII.

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* From a comparison of the observations on the temperature of a spring in a well sunk in London, which was constant, and indicated 53°.96, with the mean temperature of that capital, the difference appears to be for that locality 3°.6 to 4¼°.—Annales de Chim. et de Phys., vol. xxi. p. 316.

CHAP. VI.—*Can the Mean Temperature of a place be determined from the Temperature of Springs? and is the Mean Temperature of the Soil the same as that of the Air?*

Roebuck seems to have been the first who called attention to the accordance of the mean temperature of the air with that of the soil. He found, by three years' observation, that the former is for London 52°.16, for Edinburgh 47°.68, and adds that the mean temperature of the springs in London is 51°.01, and in Edinburgh 46°.985.*

At the time when Wahlenberg made his observations, the hypothesis of an increase of temperature towards the centre of the earth was not yet so generally adopted by philosophers as it is at present. It was, consequently, not taken into consideration, that the mean temperature of the soil can only be learnt from springs which do not sink below the limit of the influence of the external temperature. At a later period, it was considered plausible to assume, that only the mineral springs rose from a more or less considerable depth, and that all pure water springs had their origin near the surface. But even if this be admitted, it will be found by no means an easy matter to ascertain whether a certain spring be a mineral or a pure water spring; for there exists no general mark of distinction. Neither can the contents of fixed, nor of gaseous substances, and still less the temperature, serve as the distinguishing character of a mineral spring. There are springs, indeed common well waters, which are tolerably rich in fixed substances, and, in that respect, surpass others which no one hesitates to call mineral springs, because they contain gaseous substances in considerable quantities, which are disengaged from them in greater or less abundance.[*] On the other hand, there are others, rich in fixed substances, but not containing more gaseous parts than the commonest well-water. The temperature is not in proportion to the contents of fixed substances, as has already been remarked, for cold mineral springs frequently contain more than warm ones.[†] Gastein and Pfäffers are examples of this. But with regard to their gaseous contents, the temperature of springs is, in general, in the inverse proportion, the reasons for which are already known.

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* Transactions, 1775, p. 459.

It is equally difficult to draw the line between warm and cold springs. They form an uninterrupted series from the coldest to the warmest. There certainly is no degree between 33°.8, the temperature of the coldest springs observed in Lapland by Wahlenberg, and the boiling springs of Iceland, which does not answer to some spring. The coldest springs of any place are evidently those whose mean temperature is exactly equal to that of the atmosphere; for no spring can be colder, unless its source lie in neighbouring high mountains, from which it brings down cold with it into the valleys. But as we can only become acquainted with the coldest springs of any place, by comparing their mean temperatures with that of the atmosphere, it is rather difficult, from the temperature of springs, to deduce the mean temperature of the earth or of the atmosphere at the place of their origin. However, if we observe the temperature of several springs in any place, during a whole year, the mean temperature of the coldest of them will give a maximum which the mean temperature of the place cannot surpass; and in many cases, that maximum will be the real mean temperature of the place. That the observations on the temperature of springs must be continued for at least a year, in order to lead to any conclusions, is evident; for in the hottest seasons, the spring of which the yearly mean temperature is the lowest, will often be found to be warmer than a neighbouring thermal spring whose temperature only exceeds the mean temperature of the place by a few degrees. From the temperature of springs which are constant throughout the year, or nearly so, certainly no conclusions can be drawn respecting the mean temperature of the place; for as there exist thermal springs—as we have seen in the foregoing chapter—of variable temperature, it cannot be expected that one of the coldest should have a constant temperature. It may, therefore, be assumed with safety, that the mean temperature of a place is always below that of a spring whose temperature is constant at different seasons of the year.

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[*] See Bischof in the Journ. für Prakt. Chemie, vol. i. p. 334. Note.

[†] Ibid. p. 340. and 341.

At the first view, it would seem that deep wells were particularly suitable for determining the temperature of the soil. Many objections may, however, be called to mind against this. Von Buch remarks, that only such wells are proper for this purpose as are in constant use, by which their waters are kept continually in circulation; but not such as remain undisturbed, for in them the cold air sinks down from the atmosphere, and cools their walls below to a greater extent than the propagation of heat would allow. Farther, it must be presupposed that such wells are only supplied by waters filtered through the superficial strata of the earth, and that they are so far distant from any mountains of importance, that it can no longer be considered possible for waters to filter down into them from higher and colder regions. But it must also be supposed that they do not receive any tribute from warmer springs rising from a greater depth. If, in sinking a well, a stratum be met with which is impervious to water, and which has no where been broken through, one may be tolerably certain that the latter case has been avoided. A communication with clefts, by which they might be supplied with waters from below, in the manner of artesian wells, is most to be apprehended when a well is cut in the solid rock. Deep wells, which communicate with neighbouring deep rivers, are very likely to hold thermal waters, especially if there be any rising ground between the springs and the river, although it be but a few hundred feet above the bottom of the valley. Thus the temperature of a well of fifty-eight feet under my laboratory, which is in connexion with the Rhine, is almost constant, and is about 1°.35 higher than the mean temperature of the soil here. This spring is consequently thermal. The mean temperature of the soil cannot, therefore, be deduced with certainty from observations on the temperature of wells. An approximate result can only be obtained by extending the ations to many different wells.

If we apply these general conclusions to Wahlenberg's observations, it is easy to perceive that he has determined the mean temperature of the soil at Upsala, at the Ynger See, at Umeo, Söderköping, and Carlscrona, in all cases too high, because he deduced them from observations made either on constant warm (thermal) springs, or on variable springs in months in which they could not possibly have their mean temperature. We must therefore call in question his conclusion, that the mean temperature of the earth in the north is every where higher than the mean temperature of the air, and that the difference between them is greater the higher the latitude, or the colder the winters.

It was shewn in chap. ii. how universally thermal springs are dispersed over the earth. In rocks which are very much dislocated, and in which the atmospheric waters descend to a considerable depth, the number of thermal springs may probably surpass that of the cold ones; indeed, perhaps, scarcely one will be found from which the mean temperature of the soil can be determined. I found this assertion upon numerous observations which I made in April 1833, and May 1834, upon the springs at the foot of the chalk rocks of the Teutoburger Wald, in strata which are very much fissured. In April 1833, I found among sixty-six running fresh-water springs, only three whose temperature was below $47\frac{3}{4}$°; all the rest varied from $47\frac{3}{4}$° to 60°.6. In May 1834, the temperature of those three springs had risen 3°.6. As, according to the table, no spring, as yet observed, reaches its mean temperature earlier than April, and most springs not until June, and sometimes even July, the mean of those three springs cannot fall below $47\frac{3}{4}$°. If it falls above $47\frac{3}{4}$° they may be considered as thermal, for the mean temperature of the air at that place cannot be much above $47\frac{3}{4}$°. Another circumstance in favour of this is, that, in May 1834, I found a fourth spring near those three, the temperature of which was $47\frac{3}{4}$° or 3°.6 colder than the coldest of the others. If this fourth spring ob serves the same law in the variations of its temperature as the other three do, there is no longer any doubt that they are thermal. There can be no question whatever that the other sixty-three springs are thermal.

Besides those sixty-six fresh-water springs, I observed in May

1834 a great number more in that neighbourhood; and, on similar grounds, I think that I am justified in considering the majority of them as thermal.

In mountains which are split to a great depth, it is therefore possible that scarcely one spring may be found from which the temperature of the soil might be determined. And, even when such is not the case, an approximative idea of the mean temperature of the soil can only be obtained, provided there be no high mountains near, by observing the temperature of a great number of springs in the neighbourhood for at least a year, and taking the coldest among them.

So long, however, as no springs are found whose differences of temperature are in totally different proportions from those in the table, a tolerably correct value may be found for the mean temperature of the soil, from observations on the temperature of single variable springs, if made at a well chosen season, and provided they do not rise out of unusually fissured rocks. For instance, since the mean temperature of springs generally falls in the months of December or January, and in June or July, it is sufficient to observe the temperature of springs only at those times, in order to get a very near approximation to the actual mean temperature of the soil. It may be ascertained still more exactly, by finding out exactly the period of the maximum or minimum of the springs, and observing their temperature three months after that date. The precise period of the mean temperature of springs for each individual case, cannot be determined with such exactitude, as it was for the mean temperature of the air, by the observations made at Leith in Scotland.*

The same objections which we have made to Wahlenberg's determinations of the mean temperature of the soil, are also applicable to the observations of Kupffer† in several parts of eastern Russia, as well as to the conclusions which he was led to draw from them.

Erman jun.‡ finds the temperature of the springs at Königsberg 3°.55 higher than that of the atmosphere. I can only account for this great difference by supposing the temperature of the air at Königsberg to have been probably determined much too low; for at Mitau, which lies 2° farther to the north, and rather to the east, it has been found 1°.35 higher.*

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* Results of the Therm. Obs. made at Leith Fort every hour of the day and night during the years 1824 and 1825, p. 19.

† Poggendorf's Annalen, vol. xv. p. 159, and following.

‡ Poggendorf's Annal. vol. xi. p. 310.

The temperature of the springs near Berlin and Potsdam strengthened Erman sen. et jun. in their opinion, that in high latitudes the temperature of the soil is higher than that of the air. They estimate the temperature of the soil at Berlin and Potsdam at 50°.07 to 50°.15, and consequently find, as Humboldt and Tralles ascertained the mean temperature of Berlin to be 46°.4 to 47°.3, that there is a considerable difference between their results.

If the mean temperature of the soil can only be ascertained from the coldest springs (provided there be no high mountains in the vicinity which could send down cold springs into the valleys), and if, without exception, all constant, or nearly constant, springs are thermal, then even the Louisenbrunnen and the spring at Templin must be thermal. With regard to the latter it is the more probable, as it is indebted for its existence not only to the meteoric waters of the neighbouring hills, but seems to take its rise at a much greater distance. The mean temperature of the earth at Berlin is therefore probably below 49°.1, so that the difference is considerably diminished, if not totally annulled. The difference of 2¼°, mentioned by Erman, between the temperature of the springs in Freienwald and at Neustadt is also a corroboration of this opinion.

* From the six years' observations made at Stuttgart on the temperatures of the air and of the springs, it was found that the mean of the latter only surpassed that of the former by 0.°427. It must, however, be observed, that the mean temperature of the air was determined too high, on account of the want of observations made during the night; but then, on the other hand, that of the springs must also have been estimated too high, as the observations were made at the time of the daily maximum temperature of the atmosphere, and as the wooden aqueduct, which is about a mile in length, lies at a depth of only three feet, and in several places is made to pass through brooks. And if the influence of the daily variations of temperature be no

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* Ibid. vol. xv. p. 1[illegible]3.

longer felt at the depth of three feet, as Munke's[*] observations seem to shew, still heat would be imparted to it by the rivulets through which it passes. It is therefore very possible that the mean temperature of the spring was estimated as much too high as that of the air was.

I must here call attention to another circumstance, which is certainly worthy of notice. Observations on the temperature of the air are generally made in places protected from the direct influence of the sun's rays, as well as from radiated heat; but the soil is exposed to these. The yearly mean temperature of the air must then, if ascertained in such a situation, be lower than the mean of the superficial crust of the earth; but variable springs receive their heat from that external crust, so that their temperature must be higher than that of the atmosphere. Single observations on the temperature of the soil and of the air have given very considerable differences. Thus, Humboldt[*] found the temperature of the air on the Orinoco at two o'clock to be 86°; that of a coarse moveable granitic sand, 140°.45; that of a similar white, but close-grained, fine sand, 126°.5; and that of the granite rocks, 117°.725. An hour after sunset, the coarse sand showed 89°.6, and the granite rock, 101¾°. As sand absorbs the heat of the sun in a greater degree than other earths, and as the meteoric waters, filtering slowly through it, easily assume the temperature of the sand, it is very possible that the mean temperature of springs which have their origin in a sandy soil may rise higher than the mean temperature of the air. This may, perhaps, be the case with the springs near Berlin. For that reason, it would be very desirable to combine observations on the temperature of the superficial strata with those made at the same time on the temperature of springs and of the air.

The thermometrical observations at Tübingen were made in a well of running water in the botanical garden. A comparison of the yearly mean of these with the yearly mean temperature of the air, speaks in favour of the opinion that the former is higher than the latter, for they give—

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[*] Gehlers neues Physikal. Wörterbuch, vol. iii. p. 988. Fourier's "Théorie de la Chaleur," places this limit at about 3 yards.

[†] Voyage, vol. vii. p. 203.

		Mean Temperature of the	
		Spring.	Air.
1831,	.	50°.47	48°.65
1832,	.	49°.68	45°.00
1833,	.	49°.23	46°.90

But, if we compare the mean temperature of the springs at Stuttgart with that of the spring at Tübingen, we shall be inclined to suspect that the latter is thermal, for this place lies higher, and in a less mild climate, than Stuttgart, and yet the mean temperature of the spring at Tübingen was somewhat higher than that of the springs at Stuttgart.

The uniform nature of the soil in Basel, out of which rise eight fresh-water springs, in the space of about half an English mile, offers a particularly favourable opportunity of observing the temperature of the earth in that locality. The soil of Basel is covered with considerable deposits of rolled stones, under which there is a bed of marl and clay, falling gently to the north-east towards the Rhine. The meteoric waters, after having filtered through the layers of loose stones, meet with this impervious bed, the gentle slope of which directs them towards the Rhine.

Among these springs the Lochbrunnen, on the Herbrigberg, is remarkable for its low temperature. To my question whether this low temperature might not perhaps be derived from the neighbouring heights of the Jura, Professor Merian answered that such a conjecture could not be borne out by the circumstances of the locality. He is much rather inclined to ascribe the low temperature of that spring to the phenomenon observed by Forchhammer on the Faroë Islands, viz. that those springs which rise out of loose stones are invariably colder than those which rise out of the solid rock at the same elevation. (See a subsequent chapter.) On the other hand, the St Alban Thalbrunnen may be considered as a thermal spring. If these two be excluded, and the mean deduced for the rest, we obtain 49°.26 for the mean temperature of the earth at Basel. According to the comparison made by Merian, with the mean temperature of the air at Strasburg, and at Geneva, this value can differ but little from the mean temperature of the air at Basel.

As it was assumed that in high latitudes the temperature of the earth was higher than that of the air, so it was conversely supposed, that in lower latitudes, the mean temperature of the springs was somewhat lower than that of the air. This supposition was founded particularly upon Humboldt's observations between the tropics. But that philosopher himself remarks*, that in steep and high mountains, where either the snow-water quickly mixes itself with lower springs, or where they rise near their origin in lofty regions, the mountain springs show a lower temperature than the mean of the place where they burst forth. He refers for examples of this to the above mentioned observations in Jamaica, and to his own in the mountains of Cumana and Caraccas. Von Buch† adds to these, similar observations on springs near the Havannah, and in the interior of Congo, and remarks, that the inferior temperature of springs begins to shew itself already in the south of Europe; and that there are probably many springs in Portugal, Spain, and Italy, which differ in their constant temperature from that of the air, much more widely than the springs in tropical countries. Thus, for example, he refers to a spring at St Cesareo, not far from Palestrina, near Rome, the temperature of which he found, on the 29th of August, to be 53°.37, whilst the mean temperature of the air at the same place, is 60°.35. But may not this low temperature have been brought down with it from the neighbouring Apennines, which, in the immediate vicinity, attain a height of 2000 to 3000 feet‡?

It is easy to prove that it is contradictory to the hypothesis of an increase of temperature towards the centre of the earth, to suppose that the temperature of the soil in low latitudes is lower than the mean temperature of the air; for if the temperature of the earth's crust in those regions were lower than that of the air, there must be a stratum at a certain depth, whose temperature would be equal to the mean temperature of the air. Thus the external crust of the earth would be inclosed between two strata, the one of air and the other of earth, both of which would be warmer than itself. A permanently low temperature of the external crust can be conceived in no other way, even considering it to be a much worse conductor of heat than it really is, than by supposing the existence of a never-failing cooling principle; but, then, what could that principle be? Such an hypothesis is shewn to be still more contradictory by the above-mentioned observations of Boussingault; namely, that, between the Tropics, the temperature of the earth is constant at the depth of one foot below the surface. For how can it be supposed that the crust of the earth should, at the depth of one foot from its surface, have a constant temperature lower than that of the air, with which it is continually in contact? But that supposition is most completely refuted by Boussingault's comparative observations, as will be seen from the following table:

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* Gilbert's Annalen, vol. xxiv. p. 45 and 46.

† Poggendorff's Annal. vol. xii. p. 407.

‡ From a personal communication of Professor Hoffmann, I have learned that a great number of springs, in the deep valley of Teverone, between Tivoli and Subiaco, are remarkable for their low temperature, which amounts, on an average, to from 47¾° to 52¼°.

	Mean Temp. of the Air.	Temp. of the Soil, one foot below the Surface.
Zupia,	80°.375	80°.37 to 80°.60
Mines of Marmato,	78°.125	77°.67 to 78°.12
Anserma Nuovo,	85°.55	85°.10 to 85°.55
Popayan,	74°.075	72°.95
Pasto,	64°.85	65°.07
Quito,	66°.987	66°.42 to 66°.87

Between 5° south and 11° north latitude, the mean temperature of the air exactly accords with the temperature of the earth, taken at one foot below the surface of the earth, in a place protected from the sun by a roofing.

In order to obtain a similar comparison in the temperate zones, it would be very desirable that similar observations on the temperature of the soil should be made in our latitudes which, however, it would be necessary to continue for at least a year. Till now, little has been done towards this object. According to the observations of Rudberg, the mean temperature of the earth at Stockholm is 1°.62 higher than that of the air. Three years' observations, conducted by Herrenschneider at Strasburg, gave the mean temperature of the air 0°.54 higher than that of the soil. The mean of each year, however, differed as much as 1.665 one from the other. It is, therefore, necessary for the comparison that the observations on the air and on the soil should both be made in the same year. For this reason, no importance can be attached to the above differences.

Taking all circumstances into consideration, we find that there are grounds for assuming, that in high latitudes the earth has a higher, and in low latitudes a lower, temperature than the mean temperature of the air. The springs in high latitudes which led to this assumption were, therefore, without doubt, thermal, that is to say, springs which receive their heat from the interior of the earth. On the other hand, those springs which led to the conclusion that in low latitudes the temperature of the soil is lower than the mean temperature of the air, seem to have been mountain-springs, which brought down their lower temperature from higher regions. Boussingault remarks in general, that in the Cordilleras there are often no springs to be met with for a distance of several hundred miles. Wherever he did find any, for example at Santa Marta and at Cartagena, their temperature exactly corresponded with the mean temperature of the air. It is, therefore, the more plausible to suppose, that the few springs of which the temperatures have been determined in those countries, have only been found in mountainous districts.

Kupffer's *isogeothermal** lines, drawn through all points of the earth's surface which have an equal temperature of soil, an imitation of Humboldt's *isothermal* lines, lose their importance in consequence of the above considerations, as, in general, isogeothermal and isothermal lines coincide.

There is, however, no doubt that differences will here and there be found between the mean temperature of the air and that of the soil. Thus, in valleys which are surrounded by high and steep mountains, the temperature of the soil may be rendered lower by springs which bring down cold from above. In this case, the mean temperature of the air will be higher than that of the earth. The severe climate of certain mountain-valleys may perhaps proceed from this. In like manner, as the highest mountains on our globe are situated for the most part in low latitudes, it is to be expected that the temperature of the earth would there be lower than the mean temperature of the air, rather than in high latitudes. Thus the hypothesis of a lower temperature of the springs than of the air, may hold good for several points in the mountainous parts of the torrid zone.

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* Poggendorff's Annalen. vol. xv. pp. 190 and 183.

On the contrary, the temperature of the soil will be raised if the rocks be so fissured and cleft as to admit of the rising of thermal springs. If such waters rise in considerable quantities, and with a great heat, they may cause the temperature of the earth to rise perceptibly above the mean temperature of the air. Thus, the temperature of the soil at Aachen and Burtscheid, where waters of 90° to 112°.5 higher than the mean temperature of the air spring up in large quantities, is probably higher than that mean. The same may be expected at Paderborn, where, according to my measurements, more than a million pounds of water are ejected in a minute by all the Badersprings together, which are 6°.75 warmer than the mean temperature of the air. The earth's crust being cleft to a great depth, and thus allowing warm springs to rise, may therefore cause an elevation of the temperature of the soil.

But the temperatures of the soil and of the air must by no means be invariably considered as identical. Copious springs, heated to any degree above or below the mean temperature of the air, and rising in great numbers, communicate their temperature to the strata through which they flow. Single and scantily supplied springs can, however, cause but trifling alterations. The above mentioned springs in the Teverone valley, between Tivoli and Subiaco, whose temperature is only 47°.75 to 52.°25, and whose number is very great, although they yield but little water, must certainly tend but slightly to cool the earth, for in this valley all the products of middle Italy, as the vine and the olive, flourish most luxuriantly.

There is another phenomenon, which, although it is only local, must not escape notice. It is well known that in certain places there are cavities beneath the surface, in which there is ice both in summer and winter. The mean temperature of the ground in such caverns cannot be above 32°, whilst that of the atmosphere may be much higher. This is not the place to examine how such natural ice cellars are caused; it is only necessary for us to point out their frequent occurrence.

If we do not confine ourselves to places where there is ice throughout the year below the surface, but include all places of which the mean temperature is lower than that of the surface (and in fact they only differ from the former as to the degree), we shall with a little attention easily increase their number.

De Saussure* has made us acquainted with several subterranean caverns out of which wind issues, which is colder than the mean temperature of the soil, namely, the caverns of Monte Testaceo near Rome; the Ventarola della Funera on the island of Ischia, which lies still more to the south, and is so entirely volcanic, and full of hot springs; the caves of St Marino and of Cesi; the Cantines of Chiavenna; the caverns of Caprino on Lake Lugano; the cold grottos of Hergisweil, near the lake of Lucerne, and so forth. It is true De Saussure only gives us single observations of the temperature of these caverns, made in the summer months; but the observations of Nollet in the grottos of Monte Testaceo were made in September. These seem to shew that the yearly mean temperature of those winds is much lower than the yearly mean temperature of the air, and that their temperature rises as the seasons advance.

In the Saxon Erzgebirge, Reich† mentions three points which are remarkable for their low temperature. The Heinrichssohle in the Stockwerk at Altenberg was found by two years observations to have a mean temperature of 0.°54 colder than that of the surface, although situated at a depth of 400 feet. In the Henneberger Stolln (Adit of Henneberg) on the Ingelbach near Johanngeorgenstadt, Reich found the temperature about 0.°54 lower than in the Gnade Gottes and Neujahrs Maassner shaft some hundred feet higher. The Weiss Adler Stolln on the left declivity of the valley of the Schwarzwasser, above the Antonshütte, is also remarkable for its extraordinary cold.

Reich‡ has also communicated some interesting observations on the perpetual ice in the mines of the Sauberg, at Ehrenfriedersdorf. He determined the mean temperature of the surface of the Sauberg at 44°.94. The result of several years' observations in the St Christoph shaft, which lies to the west, and is the least exposed to the cold, was 42°.22, at thirty-one feet below the surface; the same temperature was found in the Morgenröther cross-cut, lying to the east of this shaft, at a depth of 281 feet from the surface. The temperature is, then, considerably lower than the mean temperature of the surface, as well near the surface as at a greater depth. In the Sauberg the ice is seldom found lower than fourteen fathoms; however, it was once observed in the interstices of the Old Man, as much as twenty-four fathoms below the surface. Reich found the temperature of the ice to be 31°.982, the air in the immediate vicinity 34°.025, and the rock at a little distance 32°.765.*

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* Voyages dans les Alpes, vol. v. p. 342, with remarks by Nicholson, in his Journal of Nat. Philos. &c. No. 5, 1797, in Gilbert's Annalen, vol. iii. p. 201.

† Beobachtungen ueber die Temperatur des Gesteins in verschiedenen Tiefen in den Gruben des Sächsischen Erzgebirges, in den Jahren 1830–1832, &c. Freyberg 1834, p. 200 and following.

75, and following.

At many other places, in the Erzgebirge, the temperature of the soil was found, on the contrary, to be higher than that of the air, viz. :—

	Soil.	Air.	Difference.
Altenberg, ...	42°.738	41°.276	1°.462
Markus Röhling, . .	43°.542	41°.832	1°.710
Johanngeorgenstadt, .	43°.115	41°.090	2°.025†

At Markus Röhling the temperature of the soil, as well as of the air, is remarkably low, in comparison to its elevation above the sea; the cause of both must probably lie in the locality. The conclusion drawn by Reich, from the above observations, viz. that, in the Erzgebirge, the temperature of the soil is throughout 1°.8 higher than the mean temperature of the air, seems, however, to be somewhat too hazardous.

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* For further information on ice-caverns and ice-grottos, see Gehler's Neues Physik Worterbuch, vol. iii. part i. p. 160, and following; and Reich, p. 188, and following. It is related of several such ice-grottos, that the ice is only there in summer, and that it thaws away in winter. By referring to the three years' observations on the temperature of the St Christoph shaft, we find that the maximum occurs in November, and the minimum in April; so that the opinion that the mines are colder in summer than in winter is easily accounted for. If a spot could be found, whose mean temperature was exactly 32°, ice would be found to continue to increase there till June or even July; and, on the other hand, to decrease until December or January. But the one would nevertheless be the effect of the preceding winter, and the other that of the preceding summer. Also, according to the observations of Dr Oudot (Journ. des Mines, t. iv. No. 21. p. 65), the ice in the celebrated cavern of Besançon, near La Chaux, increases till April, and decreases till October: in January the temperature falls to 20.°¾, in summer it rises to 38°.¾. We see, then, that these variations correspond very nearly with those observed in springs, which is nothing else than might be expected.

† Reich, p. 128.

All these facts prove that great differences may exist between the mean temperatures of the air and of the soil; but that they only originate in local causes, and do not stand in any relation with the geographical latitude of the place.

(To be continued.)

Analysis of a Memoir on the Structure and on the Origin of Mount Etna. By M. L. Elie de Beaumont, Member of the Royal Academy of Sciences of the Institute.* Communicated by the Author.

In the first chapter of the memoir, after having referred to the numerous investigations of which Etna has been the object, I have indicated the precise point of view under which I undertook to examine the subject. My purpose was chiefly to ascertain and to explain, more precisely than had been previously done, the orographical "accidens" which have modified the regularity of the pyramid of Etna.

I must necessarily commence by giving an account of the general features of the form of the mountain. In order to illustrate this part of the subject, I have the honour of presenting to the Academy a map, four views, and a relief model of Etna, which I have constructed in part from my own observations. If I had succeeded completely in the execution of these different objects, they would have presented to the eye a complete analysis of this part of my essay; but even in that case I could not have dispensed with defining some expressions, and particularly some of the leading features. It will be found, doubtless, that the map, the views, and, above all, the relief model, correspond in a very small degree with the poetic image of Pindar, who termed Etna "*the pillar of heaven.*" But this very flatness of the mountain, of which it is easy to perceive that the representation does not, at the present day, preserve any thing that is imaginary or arbitrary, appears to me destined to become, in the eye of science, one of the most striking features of its form. This flatness, if well analyzed, would of itself be already almost a theory. The sea and the rivers Onobola and Simeto bound nearly completely the mass of Etna. A "falaise" more or less distinctly developed, marks the circumference of the region round nearly its whole extent. At the summit of the "falaise" commences a platform which is slightly convex; and this again is surmounted by a very depressed cone, whose acclivities, which may be termed *lateral taluses*, terminate on all sides at the foot of an irregular gibbosity, which forms the mountain properly so called. This latter *central gibbosity* is itself truncated by a surface nearly smooth, termed the *piano del lago*, on which is elevated, like a sugar-loaf, the notched cone that is terminated by the crater of the volcano. The *central gibbosity* is not a cone, but bears a strong resemblance to the remaining portions of one of which a part had disappeared. Its most massive and elevated portion, surmounted by the *piano del lago*, presents as it were a trunk, whence, to use the expression of the Canon Recupero, branch off two arms slightly curved the one towards the other, which embrace a space having a rude elliptical form, and within whose limits are prolonged the taluses, having their usual inclination and regularity. These two arms are narrow crests almost sharp, sometimes toothed, and whose two declivities are unequal. The exterior declivities, although steep, never form escarpments; indeed, they rarely attain an inclination of 32° to the horizon. The interior declivities, on the contrary, which face one another are abrupt, and often even perpendicular for heights of several hundred feet. The space which they circumscribe, and which is called the *Val del Bove*, is an enclosed amphitheatre, whence there is no view of objects beyond its limits except in the direction of the sea. It is on the flanks of this vast abyss that we see written in indelible characters the history of the commotions which have given to Etna its particular form, and whose meaning it is the object of my memoir to decypher.

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* Read at the French Academy of Sciences Nov. 30. 1835.

We have great pleasure in presenting to our readers this important memoir, communicated to us in manuscript by its distinguished author.—Edit.

I shall only pause an instant to present some details that possess an interest which is in some measure collateral.

On the 19th September 1834 I ascended to the summit of Mount Etna with M. Leopold de Buch, Professor Link, M. Achille Richard, and several other savans. It has appeared to me proper that a narrative of this excursion should form part of the documents to warrant and support my investigation; but, in the present analysis, I shall only extract from this account the two following remarks.

It has been ascertained that the greater number of the appearances of flames which accompany volcanic eruptions, are only the effect of the rays of light which emanate from the incandescent lava, and which are reflected by the molecules of vesicular vapour and of dust disseminated by the eruption in the atmosphere. In consequence of this observation, doubts have been raised as to whether volcanos, in any case, produce real flames. These doubts have been already removed by Sir H. Davy in regard to Vesuvius, where he ascertained, during a small eruption, the existence of a real jet of flame; and we ourselves have observed on Etna incontestable volcanic flames. Having left the *Casa inglese* about an hour and a half before daybreak, in order to ascend to the edge of the crater, the feeble light of the stars enabled us to perceive, on the commencement of the acclivity of the upper cone, a white space whose colour was caused by the alteration of the rocks, and by saline efflorescences having a very styptic taste. In the midst of this space, at several points, we distinguished pale and scarcely luminous flames, which seemed to issue from the earth; they occupied the orifices of several irregular openings, which were from one to two yards in width, and were only the enlargements of a tortuous crevice. These flames were evidently produced by a gas disengaged from the crevice, and which did not find the oxygen necessary for its combustion till it reached the external air. The combustion took place almost exactly at the level of the surface of the ground. The flame rarely rose to the height of a yard; it produced a sound somewhat intermittent, pretty analogous to that of several lighted fagots, or rather that which is heard at the bottom of a blast-furnace when the blowing apparatus is badly constructed. The gases produced by the combustion did not impede the breathing, and had a strong odour of sulphurous acid. Sulphuretted hydrogen was also perceptible, but I did not recognise the odour of muriatic acid. Every circumstance, then, announced that the flame was supported by sulphuretted hydrogen, and afterwards, when the sun lighted up the mountain, a long bluish cloud was seen taking its rise from that particular point.

In the interior of the great crater I found several portions of snow, but from many other points of its angular bottom there issued hot vapours, having a whitish colour, more or less dense, composed chiefly of watery vapour, but having nevertheless a strong odour of sulphurous and muriatic acids; one or the other of these acids predominated alternately. The surfaces across which the vapours were disengaged were in part covered by saline efflorescences, which were sometimes white, and sometimes coloured of an orange-yellow tint by the chloruret of iron, or of a canary-yellow by particles of lava altered by the acid vapours. In some fissures I found white fibrous gypsum, mixed with altered pulverulent yellow lava in which some small nodules of sulphur were disseminated. In my memoir I have described the products of the eruptions of Etna, but the limits of this analysis do not permit me at present to enter into details on this subject. The products of the eruptions of Etna resemble externally those of a great many other volcanoes, but they present a peculiar composition which hitherto has not been met with except in those of Stromboli. Professor Gustave Rose, in a memoir on the composition of rocks called *grünstein* (greenstone), has published for the first time the fact that the lavas of Etna do not contain common felspar or orthose, but *Labrador felspar.* They are composed of *Labrador felspar*, *augite*, and *peridote* (olivine). The observation of Professor Rose was not known at the time of our expedition, but the true composition of the lavas of Etna could not long escape the experienced eye of M. de Buch, who pointed it out to me almost the first moment we reached the volcanic mass. My subsequent excursions afforded a constant confirmation of this first observation.

The fear of encroaching on the time of the Academy induces me to suppress also various remarks relative to the last eruptions of Etna, but still there is one fact which it is necessary to mention. On the 1st October 1834, I visited the extremity of the stream of lava which had issued from the flanks of Etna in the month of November 1832, that is, twenty-two months and a-half previously. The stream had stopped at two miles from the town of *Bronte*, on a gently inclined mass, at which point it had accumulated to the height of about twelve yards; it was still hot in its interior, and on traversing its surface little gusts of extremely hot air issuing from its fissures were every instant felt. Besides, there arose from many interstices situated chiefly on the most elevated portions of the unequal surface of the lava, little streams of watery vapour having a very elevated temperature. These vapours had a strong odour of muriatic acid; but no traces of sulphurous acid could be distinguished. They deposited on the walls of the fissures a considerable quantity of saline substances, and chiefly of muriate of ammonia, which was sometimes perfectly white, and sometimes coloured orange-yellow by the chloruret of iron. At some points the saline deposit was slightly coloured green. The muriate of ammonia was sufficiently abundant to enable the man who acted as my guide to gain a livelihood by collecting it. An attentive examination convinced me that the vapour and the saline substances were disengaged from the parts of the lava which were not yet cooled. The question as to how these substances could remain included in the melted mass during years, is a very difficult problem in molecular physics; but as to the *fact* it seems to me incontestable.

Among the phenomena presented by all great eruptions, there is one which, notwithstanding my desire to abridge, cannot be passed over in silence. These eruptions are almost always announced by shocks of earthquakes, which shake not only Etna, but often nearly the whole of Sicily. These shocks are not always confined in their effects to simple vibrations, for frequently they are sufficiently violent to fracture the mountain, which thus yields to the force acting on it from below upwards; and to separate the walls of the rents thus produced to a greater or less extent, sometimes to a distance of several yards. These rents nerally follow vertical planes, which pass nearly through the e axis of the volcanic chimney, and which cut the surface of the mountain in the direction of one of its meridians. As soon as such a fissure is formed, the lava which bubbles up in the central vent speedily enters, and almost always opens for itself there a passage, by which it flows out laterally on the flanks of the volcano, producing a lateral eruption. Many eruptions of this description have taken place at a great distance from the axis of the mountain, and at an inconsiderable height above the sea; one occurred near Catania, at a distance of six leagues from the great crater.

The lower part of each of these meridional fissures, of which I have been speaking, remains filled with lava, and a vein is thus formed. As to the upper part of the fissure, situated above the point whence the lava flows, it often becomes filled with scoriæ or debris; some, however, of these fissures have remained empty, and a part of the grottos which are mentioned as occurring in the mass of Etna have had no other origin. Near Nicolosi, there is a grotto called *Grotta dei Palombi*, whose entrance was cleared by M. Mario Gemellaro, and into which I descended. The breadth of the internal hollow varies from one to four yards, and I regret I cannot describe it at present.

In the eruption of 1832, the phenomenon of meridional fractures presented itself, accompanied by remarkable circumstances. A fissure was produced in the direction of Bronte, and thus opened an exit for the lava on that side; another rent commencing at the summit, which was till lately the most elevated, and which was at last broken down by this occurrence, crossed the *Piano del Lago* in the direction of Catania; other accessory fractures took place at the same time; and in this eruption, to make use of a common expression, the mass of Etna was completely "*etoilé*." The rent which traversed the *Piano del Lago* produced destructive effects, which, if our time permitted, would be well worthy of being particularly mentioned. But the most curious, perhaps, of the results produced by this fracture was, that, in dividing the plain termed the *Piano del Lago*, it changed the relative level of the two segments to an extent amounting often to more than a metre. On the eastern division, which has sunk in height relatively to the other, are still to be seen the ruins of a small ancient edifice called the *Torre del Filosofo*, which was built 1500 or 2000 years ago. During this lapse of time, as Brydone long ago remarked, the products of the eruptions have accumulated round the foundation of the building only to the insignificant height of two yards. The rent of which I have spoken above, in changing the relative level of the two segments of the *Piano del Lago,* caused at once a change of form more considerable than had resulted from the accumulated products of the eruptions of ages. This change of relative level proves, that Etna does not repose on immoveable foundations, and that the segments into which the meridional rents divide it, are susceptible of a certain change of position. Is the total amount of movement which the segments of Etna have undergone in consequence of the "*étoilement*" which occurred in 1832, to be regarded as a sinking or as a "soulèvement"? Measures of height would here be of great assistance, if it were possible to give them sufficient precision; but in their absence, it appears to me that the question may be resolved by a very simple mode of reasoning. The walls of the fractures being separated, it is evident that the surface of the mountain has undergone an increase of size, and that this enlargement necessarily presupposes a tumefaction. The mountain has therefore been elevated, and to an extent which might easily be calculated if the breadth and length of the fissures were exactly known. This amount of elevation is evidently very small, but still the mere existence of such a change is an important fact. The ejected volcanic matter which accumulates on the central gibbosity of Etna, increases its height with extreme slowness, since the lower part of the *Torre del Filosofo* is still visible after a lapse of 2000 years; and, indeed, extremely precise measurements would evidently be necessary, to ascertain *if, at the present day, and under our own eyes, eruptions contribute more than "soulevemens" to increase the size of Etna.*

Part II.—*Divisions of the Rocks of Etna into six Formations.*—Mineralogists have often complained of the monotony of Etna; but, in the eyes of a geologist, the mountain presents great variety. Six formations at least can be distinguished, viz.:

First, The rocks termed Primitive, which do not appear any w surface, but whose existence beneath is indicated by fragments of a granitic rock ejected by the mouths of the volcano.

The Second Formation, indicated on the relief model by the yellow colour, is composed of calcareous and arenaceous rocks, which constitute chiefly the mountains from which Etna is separated by the rivers *Simeto* and *Onobola*. These calcareous and arenaceous rocks rise at several points in the inclosed space bounded by these rivers, and the volcanic products repose on the edges of their upturned beds. I believe the greater part of these deposits may be referred to the lower chalk.

The *Third Formation*, that indicated by a blue tint, is composed of basaltic rocks, which constitute the Cyclopean islands, the hill of Molta-di-Catania, and the columnar escarpments of Paterno, Licodia, Aderno, &c. &c.

The *Fourth Formation*, coloured green in the model, includes the deposit of rolled pebbles, forming a line of hills at the junction of the plain of Catania and the first acclivities of Etna. The layers of this mass rise towards Etna under an angle of 4° to 5°, and present to it their escarpment. They seem to be referrible to one of the most recent tertiary deposits which surround the *Val del Bove*.

The *Fifth Formation*, indicated by the grey colour, comprehends the ancient lavas of which the escarpments consist. And, finally,

The *Sixth Formation*, represented by the brown and in some points by a red ochre tint, is composed of the modern ejections, whose mass is daily augmenting.

Of these six formations, the two last only present themselves in the mountain properly so called, and consequently it is their investigation which interests us more directly; but they are, at the same time, those whose component parts it would be most easy to confound mineralogically. In one, as in the other, the rocks consist of labrador felspar, of augite, and of peridote (olivine), and in the two formations the state of aggregation of these substances differs only by slight shades. But if, instead of considering these rocks mineralogically, we regard them in a geological point of view, that is, as to the general disposition of their masses, we perceive, almost at the first glance, that they form two systems, which are independent the one of the other; to employ an expression already consecrated, we recognise in the older of these two formations the summits *of an ancient world buried under a world of modern origin.*

The products of the present volcanic vent form on the mass of Etna a mantle nearly continuous, which is interrupted only at certain parts of the central gibbosity, so as to permit the more ancient rocks to appear.

This arrangement might surprise at first sight; for it would have been natural to presume that the ejected loose matters, which form the chief mass of the products of the great crater, would have covered the entire surface of the central gibbosity with a thick bed of cinders and lapilli. It is, however, sufficient to cast a glance, when the weather is clear, on the eastern part of Etna, in order to perceive that several very extensive portions of the central gibbosity have not been completely covered by the ejections alluded to; but they have only been, as it were, sprinkled over with a small quantity of these matters, which in time have been collected in the ravines, whose bottoms they mark by a black train, while all the rest is left uncovered. In the places near those where it becomes interrupted, the mantle of ejected matter is, of course, extremely thin, and we can, in fact, ascertain that these recent volcanic products are accumulated on the *Piano del Lago*, and even at the foot of the upper cone, only to a very inconsiderable thickness.

The observations already cited relative to the *Torre del Filosofo*, whose foundations only have been concealed by the eruptions of fifteen or twenty centuries, prove the extreme slowness with which modern ejected matter accumulates on the central gibbosity. The accumulation of these recently ejected matters proceeds much more rapidly on the portions of the mass of the mountain which are removed from the centre. The base of the Greek and Roman monuments, which still exist in the town of Catania, has been enveloped to a much greater extent than the foundation of the Torre del Filosofo by products of eruption. The port of Ulysses, near Catania, was overwhelmed by a current of lava, and the relief of the whole surface of the vicinity of that town has been much more changed than that of the Piano del Lago during the last 1500 or 2000 years.

It is on the lateral taluses of Etna, and on the slightly inclined platform which terminates them, that the largest mass of modern volcanic matter is accumulated. The lavas and the loose volcanic substances are there stratified bed by bed, and it is to the regular laws according to which their accumulation has taken place that we are to attribute the softness and regularity of the acclivities presented by these places. The flatness of Etna, already particularly mentioned, is the expression on the great scale of a part of these laws. The successive materials, which the eruptions add year after year to the mass of Etna, are, in fact, the elements of a cone extremely flattened, whose inclination does not exceed 8°. The extreme limit to which we can imagine that the indefinite repetition of these eruptions tends to give the form of Etna, would be a cone infinitely less elevated than that of which the nucleus of the central gibbosity seems to be the debris. If this nucleus did not exist, Etna would not be elevated at the present day above the point of meeting of the prolonged edges of the lateral taluses;—that is to say, it would not have been more than 1600 to 2000 yards in height. It is evident that the central gibbosity owes its existence entirely to the pre-existing nucleus which forms its chief mass. Far from continuing the formation of this nucleus, the eruptions of the present day tend, on the contrary, to sink it, and to make it disappear. The philosophers and geologists who, since the days of Empedocles to the present time, have seen Etna cover almost periodically its flanks with new layers of cinders, scoriæ, and lava, have admitted, almost without examination, and as a fact which was, as it were, self-evident, that the entire mountain was simply the result of the repeated gradual additions of materials, all similar to each other, and similar also to the products of eruptions taking place under their own observation. Indeed, at first sight this appears almost as natural as to attribute the entire growth of an oak to the repetition of the phenomena of vegetation which had been observed in it during one summer. But the observations and reflections, of which I now present the analysis, appear to me to prove that the whole mass of Etna cannot be reduced to elements all analogous to each other, and having a similar origin, in the same manner as the successive layers of which the trunk of an oak is composed; and that in comparing the increase in size of Etna to the growth of an individual of the vegetable kingdom, we should commit the same error as if we attributed to an ivy plant the growth of the old dead tree to whose trunk it is attached.

The features truly characteristic of the form of Etna, those in which its mode of enlargement and its first origin occur most distinctly displayed, are then, on the one hand, the feebleness and the uniformity of the inclinations presented by its base from the foot of its *central gibbosity* to the banks of the rivers and the sea-shore which surround it; and on the other, the abrupt relief, the insulated position, and the disjointed condition of the internal nucleus of that same gibbosity. The gentle slopes of the base have been produced by the deposition of debris (un remblai); but the bold projecting outline, the insulated position, and the broken up structure of the central gibbosity, owe their first origin to a *soulevement*; and such in a few words is the theory of Etna. The structure of the internal nucleus of the central gibbosity is exhibited in the escarpments of the vast elliptical amphitheatre, termed the *Val del Bove.* These escarpments are composed of many hundred layers, formed alternately of rocks of fusion, which differ from modern lavas by certain shades, and of fragmentary and pulverulent matters more or less solidly aggregated. Their thickness varies from half a yard to several yards. These layers, whose regularity is hardly ever deranged, except to a limited extent, by the crossing of veins or by other accidental circumstances, frequently form undulations which remind us of those of sedimentary beds in the high chains of mountains, and they do so without even having their parallelism or consequently their thickness altered, although in these undulations their inclination sometimes amounts to 27°. This absence of all variation in the general arrangement of the layers prevails through the entire circuit of the *Val del Bove*, and it has struck me forcibly each time I have had occasion to direct my attention to the *ensemble* of its escarpments. My observations on this subject may be summed up by saying, that the numerous layers of melted and fragmentary matters which alternate in order to form the nucleus of the central gibbosity of Etna become curved simultaneously, and pass in several different directions, from a position nearly horizontal, to an inclination of 25° to 30°, without having their structure or thickness altered in a constant manner. These layers are cut transversely by an immense number of veins of lava, sometimes vertical, sometimes more or less oblique, which, less crumbling than the beds, sometimes project beyond the escarpments like remains of gigantic walls. These veins are old fissures, analogous to the meridional rents of Etna, that have been filled by ancient lavas, and through which the melted volcanic matter, now disposed in regular layers, would seem to have issued. But notwithstanding this analogy, it must be remarked that these veins have a general tendency to an E.N.E. direction, which shews that the fractures which they have filled were not meridional fractures, standing in connexion with a central axis, and that at the epoch of the outflowing of the lava, the eruptions had not, as at the present day, a fixed and determinate centre.

The rock being cut so sharply in the escarpments of the *Val del Bove*, it is incontestable that this vast circus owes its existence to the removal of an enormous mass of matter which formerly occupied the space, or at least a great part of it. Professor Buckland, Mr Lyell, and M. de Buch have successively formed the idea that the matter now wanting must have been ingulfed in the interior of the mountain, an opinion which seems to me the most probable that can be advanced. This ingulfment may have been comparable to the falling in of the volcano of Papandayang, in the Island of Java, and to that of the cones of Carquairazo, and of Capac-urca, in the Andes of Quito. But as the lava does not rise to the external surface either in the volcanos of Java or of Quito, we have abundant latitude for conceiving that there are empty spaces in their interior; while if cavities exist under Etna, they must be filled by lava, at least during the period of eruptions, for the lava is then elevated even to the summit. The surface that the ancient lavas have covered was in this respect in the same condition as modern Etna, for the lava flowed to the surface by fissures, which were produced at certain intervals. We might in fact say that the ingulfment would take place in a cavity which the lava filled and abandoned alternately. But if the lava could issue from that cavity, could re-enter, and be submitted to a pressure capable of making it mount by the fissures to the external surface, there seems nothing impossible in believing that it may have upraised that surface. Such a soulevement would even render it much more easy to understand a subsequent "eboulement."

The question thus presents itself, Whether the masses which form the circuit of the *Val del Bove* occur at the present day in their original position, or if they do not owe to a soulevement subsequent to their formation, the outline which elevates them above the whole neighbouring country? I have been led to decide this question in favour of the hypothesis of the "soulevement" by six considerations, nearly independent of one another: viz. 1*st*, By the consideration of those veins which rise to the upper part of the escarpments of the *Val del Bove;* for, if the fissures which have given rise to these veins took place across a mass of such magnitude as the nucleus of the gibbosity of Etna of the present day, the melted matter could not have filled them to the top. 2*d*, By the consideration of the thickness of the layers which have been formed by the melted matter poured out from the openings of the veins; for a mass of melted matter flowing from the lower extremity of a fissure on a plane so highly inclined as some of the layers are at present, could only have formed a narrow current. 3*d*, By the consideration that the volcanic products given out by the openings of the veins are equally distributed on the two sides of the opening, whereas on an inclined surface they could only be spread over the side towards which the slope was directed. 4*th*, By the consideration of the invariably insignificant thickness of the layers of loose matter, which, if they had been received on a surface inclined under an angle of 27°, for example, would have slipt down, and have been accumulated at the base of the declivity to a considerable thickness, and thus have given rise to a rectilinear talus. 5*th*, By the consideration of the uniformity of the thickness presented by the layers of melted matter, even in the places where they are undulated; a fact quite contrary to the phenomena observed in modern lavas, which, when they stop on an undulating surface, present alternate enlargements and contractions. Finally, I am anew led to the same conclusion by a 6*th order of considerations,* which seems to me still more conclusive, and in regard to which it is necessary for me to enter a little into detail

The uniformity which I have particularly specified as characizing the layers of melted matter in the Val del Bove is not confined to the absence of contractions and enlargements; this uniformity pervades all the details of their structure, which is found to be invariably the same whether we observe these layers in places where they are nearly horizontal, or examine them at points where their inclination augments or diminishes gradually, or finally trace them where they present, over a great extent of surface, a general slope of 25° to 30°. Now, this want of relation between the structure of the layers and their inclination is a fact diametrically opposed to the appearances presented by all *the great streams of lava*, for in them the form constantly varies with the intensity of the inclination. The great currents of lava owe to their size itself, which permits them to spread over large surfaces, and prevents them from losing their heat for a long space of time, the peculiar laws which regulate them, and which differ more from those that regulate the lavas occurring in stripes, rounded masses, and stalactites, than the laws of a great river differ from those of a small streamlet of water.

I have endeavoured to ascertain, by a table of observations, the laws of these great streams of melted matter, and have, for this purpose, measured or calculated sixty-eight examples of inclinations of great currents, taken indiscriminately on the flanks of Etna, in the vicinity of Naples, in Auvergne, on the banks of the Rhine, in Iceland, and in the Canary Islands. I have united these sixty-eight measurements in a table, and arranged them in the order of magnitude, noting at the same time the chief features of the current to which each of the measures refers. The limits assigned to this analysis do not permit me to develop the various conclusions which may be deduced from this table, whether considered by itself, or in comparison with other numerical tables which I have also added to my memoir, viz. *a table of the inclinations of numerous currents of water; a table of the inclinations of a great many taluses of loose matter; and a table of the numerical amount of a variety of inclinations to which our eyes are more or less accustomed.* I shall limit myself to the statement, that from this table there results the conclusion, that the structure of the rocks left by a large current of lava on the surface of the ground varies according to a certain law with the inclination of that surface; *that their nature is as it were a function of the inclination.* Now, the layers of lava which enter into the composition of the flanks of the *Val del Bove* present horizontal dimensions comparable to those of the largest currents which have been thrown up by volcanos of the present day. Their mineralogical composition is almost identical with that of the lavas of modern Etna; the laws deduced from the above-mentioned table are therefore applicable to them. We ought, then, to expect that the volcanic products formed on declivities having extremely different inclinations, should have acquired also very different structures; and as we perceive that the structures of the layers which are nearly horizontal, of those whose inclination gradually increases, and even of those which are inclined on extensive surfaces at an angle of from 25° to 30°, have in all respects precisely the same characters, we are warranted in drawing the conclusion that the difference which exists in their present positions is the consequence of the movement which the layers of the one series or the other have undergone since the period of their solidification.

It only remains to determine which of the layers have changed their position,—those which are horizontal or those which are inclined. Now, if we look in the table for the place where the layers of the Val del Bove could be interposed, without, from their general characters, forming a considerable anomaly, we find that they could only be placed in the portion comprising the currents which have stopped on very gently inclined slopes. From this it is evident that the layers whose original inclination is changed, are those which are at the present day highly inclined; and that those which are nearly horizontal have, on the contrary, preserved in relation to the horizon nearly their original position.

The considerations of which I have now given the analysis unite with those which I have above simply mentioned, in proving that the portions of the layers of the escarpment of the *Val del Bove* which are highly inclined, are not at present in the position in which they were originally accumulated.

The inclination which has been acquired by some parts of this system of beds, has not been a simple movement of pressure, or the effect of dislocations purely local. It is sufficient to glance at the panoramas I have sketched, in order to perceive that the inclinations present a disposition, indicative, when taken as a whole, of a tumefaction, which, in elevating the entire mass of the central gibbosity, has communicated to the lateral portions an oscillatory movement. The surface formerly nearly flat, and now replaced by this gibbosity, has been first repeatedly fractured in various lines having a nearly constant direction. The melted matters have been poured out through the fissures thus produced, and their fluidity must have been nearly perfect, for they have flowed through rents of very inconsiderable breadth. These products were then spread on both sides of the fissures, in thin and uniform masses, similar to those composed of basalt, which in so many different countries, and especially in Iceland, are superimposed above one another, forming vast plateaus whose surface remained always nearly horizontal, in consequence of the subdivision of successive lines of eruption on an extensive space. The eruptions were, like those of the present day, accompanied by disengagements of elastic fluids, which, issuing like the lava itself from the whole extent of the fissures, carried along with them scoriæ and cinders. These scoriæ and cinders falling back like rain, both on the lava and on the neighbouring spots, produced those uniform layers of fragmentary substances, which alternate with the layers of melted matters. But at one period, it would appear that the internal agent which had already fractured so frequently the solid surface, having doubtless exerted an extraordinary energy, *broke up that surface, upraised it, and since that time Etna has existed.*

The "soulevement" does not seem to have operated here with the same degree of simplicity as in the localities where it has given rise to regular craters of soulevement, such as that of the island of Palma, or the circular walls of Teneriffe and the Somma. The effort which has elevated the gibbosity of Etna, seems to have acted, not at one single and central point, but in a straight line, represented by the axis of the ellipse of which the southern, northern, and eastern flanks of the *Val del Bove* form part; and it appears to have acted unequally on different parts of this straight line, so that its western extremity, which corresponds to the present volcanic vent, has been elevated more than all the rest. A similar soulevement would not take place without rupturing the masses so elevated, and the rents necessarily corresponded principally with the line of soulevement, or diverged in a radiating manner at its extremities,—a feature which the memoir shews is in accordance with the phenomena as they actually occur.

The elliptical amphitheatre of the *Val del Bove* presents, then, all the characters of an *irregular crater of soulevement;* but here, as in all analogous cases, the question presents itself, as to what has become of the matter which formerly filled up the now empty space of the circus, and whose extent the fractures produced by the soulevement could not nearly have equalled. I have already discussed this subsidiary question, and without pretending to decide in an absolute manner, I have announced that I coincide with the opinion expressed by Dr Buckland, Mr Lyell, and M. de Buch, who regard as the most probable hypothesis that which maintains the swallowing up of the massinthe interior abysses of the mountain; a view which seems to me so much the more probable, that it is in some measure indicated by the phenomena on a smaller scale, but still of an analogous nature, which occurred on the surface of the *Piano del Lago,* under our own observation in 1832, and at other recent epochs.

It remains to be ascertained whether this soulevement was gradual, or was effected suddenly and at once. The latter supposition seems to me the only one that is admissible. In fact, the nearly perfect resemblance which exists between the volcanic matters composing the nucleus of the central gibbosity, and those which are produced by Etna at the present day, leads us to the belief that the volcanic fire acting at the present time, is only the continuation of that which produced the ancient ejected substances. Now, the fire not being extinct, if the soulevement had been gradual there would have been a continuity and entanglement of the ancient and modern products; there would not have been that complete discordance of position between them, which constitutes one of the most striking features of the structure of Etna.

Such is, in substance, the result of the observations with which I have been occupied on the flanks of this volcano; nevertheless, I might have believed that my essay was incomplete, if I had terminated without discussing, as connected with the facts of the case, the arguments adduced against the possibility of a soulevement in volcanic rocks. But as difficulties such as I allude to cannot prevail against direct proofs, I shall terminate this analysis, which is already too long, by simply pointing out the discussion of which I have spoken to the attention of the "commissaires," to whom I feel desirous that the Academy should refer the detailed examination of my memoir.

Questions for Solution relating to Meteorology, Hydrography, and the Art of Navigation. By M. Arago.

I have somewhere read, that an individual was once lamenting, in presence of D'Alembert, that the Encyclopædia had acquired such a vast extent. You would have had much more reason for complaint, replied the philosopher, if we had drawn up a *negative* Encyclopædia (meaning thereby an Encyclopædia containing a mere indication of things, with which we are unacquainted); for in that case a hundred folio volumes would not have been sufficient.

This reply, I must admit, has hitherto appeared to me to have more point than justice. It is true that the progress of human knowledge shews us daily how far our predecessors were ignorant, and how far we in our turn will appear so to those coming after us; but the greater number of important discoveries have taken place spontaneously, without having been foreseen or suspected by any one. Thus, to cite only two or three examples, D'Alembert's *negative* Encyclopædia could not have contained the most remote allusion to that important and prolific branch of modern physics, now known under the name of Galvanism, or, as it is more properly called, *Voltaic Electricity.* The multiplicity of phenomena, likewise, which are produced by the *polarization of light,* when viewed in relation to its reflection, its ordinary refraction as well as that depending on the action of crystallized plates, would not even be indicated; and the same thing may be said of the theory of *luminous interferences,* in which the singularity of the results is not less remarkable than their infinite variety.

It must be admitted, however, that apart from those important and rare discoveries, which are made from time to time all of a sudden, or at least without any visible preparation, and give a new aspect to certain departments of science, there exist important and well-defined questions, which may be confidently recommended to the notice of observers. Having been recently called by the Academy, to draw up instructions regarding physical phenomena, with a view of being transmitted to the Commander of the *Bonite*, I soon perceived that the author of a negative Encyclopædia, even when confining himself to what is distinct and definite, would have to indicate an infinitely greater number of blanks than I was at first inclined to believe. It likewise appeared to me that published notices in relation to these were calculated to be of great utility, and that numerous well-informed persons having their time at their disposal, would receive from them an impulse which would change them from passive contemplators into active partizans of science. The readers of the present work are now therefore acquainted with the reasons which have led me to deviate from the ordinary practice, and substitute in the room of some complete theory in astronomy, physics, or mechanics, an article in which almost every thing remains to be solved, since it relates either to what we know imperfectly, or to what we are entirely ignorant of. It will remain for them to decide whether questions so drawn up will lead to the advantages I ascribe to them, or whether the trial should be confined to this first attempt. It is right, however, to inform them that the various questions successively proposed were originally, at least the greater part part of them, designed for the officers of a ship (the *Bonite*), commissioned to convey consular agents to Chili, Peru, and the Philippines; I may add, that it was intended that the circumnavigation of this vessel should commence by the way of Cape Horn, and terminate by that of the Cape of Good Hope.

Meteorological Phenomena.—In meteorology it is requisite to submit to making observations, which, at the time, are attended with no important result. It is necessary to take care to provide for our successors terms of comparison which we ourselves want, and prepare for them the means of resolving a multitude of important questions, on which it is not competent for us to enter, because the ancients possessed neither barometer nor thermome-
considerations will suffice to explain our reason for

requesting, that, *during the whole voyage of the Bonite*, note should be taken, *both by day and night, and from hour to hour*, of the temperature of the air, of the temperature of the surface of the sea, and of the atmospheric pressure. They will likewise authorize us to hope that these observations will continue to be made with the same zeal, of which an example has been given by the officers of the *Uranie*, the *Coquille*, the *Astrolabe*, the *Chevrette*, and the *Loiret*. At the same time, if unforeseen circumstances require the omission of part of this labour, it would be desirable that the sacrifice should first be made of what is least essential. The details upon which we are about to enter, seem to us calculated, in such cases, to guide the selection to be made by the commander of the expedition.

Observations designed to characterize the present state of the Globe in regard to Temperature.—Has the earth arrived at a permanent state with respect to temperature? The solution of this important question seems to require only the direct comparison of the mean temperatures of the same place, taken at two distant periods. But when we take into account the effects produced by local circumstances, when we consider to what an extent the neighbourhood of a lake, of a forest, of a naked or wooded mountain, of a sandy plain, or one formed of meadows, may modify the temperature, every one will perceive that such thermometrical data alone will not be sufficient; that it is necessary, besides, to ascertain that between the periods in question the country, and even the districts adjoining it, have undergone no important change in their physical aspect and in the nature of their cultivation. It is thus seen that the question becomes singularly complicated, and although numerals are adduced, with sufficient precision to admit of a definite estimate, they become mingled with vague suspicions, which continually throw a scrupulous mind into a state of suspense.

Is there, then, no means of solving the difficulty? These means exist, and are by no means of a complicated nature, for we have only to observe the temperature *in the open sea, at a great distance from continents.* If, for this purpose, we make choice of the equinoctial regions, it is not necessary that the observations should be continued for a series of years; the maxima temperatures observed in crossing the line on two or three occasions will be quite sufficient. In the Atlantic, the extremes of these temperatures, as hitherto determined by numerous navigators, are 27° and 29° of the centigrade thermometer (that is 80.8° and 84.2° of Fahr.) Taking into account errors in graduation, every one will perceive, that, with a good instrument, the uncertainty of a single observation of the maximum of temperature in the equatorial parts of the Atlantic Ocean, cannot much surpass a degree, and that the constancy of the mean of four distinct determinations may be relied on to a small fraction of a degree. Here, then, is a result easy to be obtained, directly connected with the calorific influences on which the temperature of the earth depends, and as much separated as possible from the effects of local circumstances. It ought to form a meteorological gift, which every age should be anxious to bequeath to that which succeeds it. The officers of the *Bonite* will certainly not neglect this part of their instructions. The excellent instruments with which they are furnished warrants us to expect all that accuracy and precision which the present state of science demands.

Of the Calorific Action of the Solar Rays viewed in their relation to the situation of places on the globe.—Animated discussions have taken place among meteorologists regarding the calorific effects which the solar rays may produce by means of absorption in different countries. Some adduce the observations that have been made towards the arctic circle, from which this singular consequence seems to result, *that the sun has a more powerful heat in high than in low latitudes.* Others refuse to admit this result, on the pretence that it is not proved. The observations made at the equator do not appear to them sufficiently numerous to be taken as one of the terms of comparison; and it is thought, besides, that these observations were made under unfavourable circumstances. This investigation might therefore be recommended to the officers of the *Bonite.* To execute it successfully they would have need of two thermometers, the reservoirs of which, on the one hand, absorb the solar rays unequally, and, on the other, are not too sensible to the cooling influences of currents of air. This double condition may easily be obtained, if, after having procured two ther- [illegible] in every respect alike, the bulb of one of them be covered to a certain thickness with white wool, and that of the other with an equal quantity of black wool. These two instruments, exposed to the sun side by side, will never indicate the same degree; that with the black covering will mount highest. The question, therefore, will consist in determining if the difference of the two indications is less at the equator than at Cape Horn, or at any other higher latitude.*

It will easily be understood that comparative observations of this nature ought to be made at equal altitudes of the sun, and during the most serene weather. Slight differences of altitude, however, will not always impair the accuracy of the observations, if care be taken, under different latitudes, to determine according to what progression the difference of the two instruments increases from sunrise till mid-day, and diminishes from the latter period till sun-set. Days on which the wind is very high ought to be altogether excluded, whatever be the state of the atmosphere in other respects.

Another observation somewhat analogous to that of the two thermometers differently covered, will consist in determining the maximum temperature which the sun imparts to a dry soil in equinoctial countries. At Paris, in August 1826, during a serene state of the sky, we found that a thermometer lying horizontally, and having its bulb covered with one millemeter of very fine vegetable mould, stood at + 54° (129°.2 Fahr.). The same instrument, covered to double that depth with river sand, indicated only + 46° (114°.8 Fahr.).

Experiments to be made on the Radiation of the Sky.—The experiments which we are about to propose ought to give, all other things being equal, the degree of the atmosphere's transparency. This transparency may be appreciated in a manner in some sort inverse and not less interesting, by observations on nocturnal radiation, which are likewise recommended to the commander of the *Bonite.*

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* There are other means still more exact for resolving the problem to which the calorific action of the solar rays has given rise; but these depend on instruments which were not to be found in the hands of our artists at the time of the departure of the Bonite, and therefore are not alluded to in the instructions of the Academy. We will return to the consideration of them on another opportunity.

It has been known for half a century, that a thermometer placed under a clear sky, on the grass of a meadow, indicates 11°, 12½°, or even 14° Fahr. less than a thermometer, in every respect similar, suspended in the air, at a few feet from the ground. But it is only a few years since an explanation of this phenomenon was given; for it was only in 1817 that Wells established the fact by means of important experiments, and in a thousand different ways, that this inequality of temperature is caused by *the feeble radiating power of a clear sky.*

A screen placed between certain solid bodies and the sky prevents them from cooling, because the screen intercepts their radiating communications with the colder regions of the atmosphere. The clouds act in the same manner; they take the place of the screen. But if we distinguish every vapour which intercepts the solar rays coming from above, or the calorific rays ascending from the earth towards the sky, by the name of a cloud, it cannot be said that the atmosphere is ever entirely free from them. The only difference is their greater or less density.

These differences, however slight they may be, may be indicated by the degree of cold to which solid bodies are reduced in the night; and this accompanying peculiarity is worthy of observation, that the transparency measured in this manner, is the *mean transparency* of the entire firmament, and not that alone of the circumscribed region which may be occupied by a single star.

In order to make these experiments under the most favourable conditions, it is obvious that we must choose bodies which cool most by radiation. According to the researches of Wells, swan-down is the substance that ought to be selected. A thermometer, having its bulb surrounded with this down, should be placed on a table of painted wood supported by slender feet, in a situation where nothing intercepts the view to the horizon. A second thermometer, with the bulb naked, should be suspended in the air at some height above the ground. With regard to the latter, a screen will secure it from all radiation towards the sky. In England, Wells obtained a difference of 15° Fahr. between the indications of two thermometers placed in the manner described. It would certainly be strange, if less important differences were to result from them in equinoctial countries, : been so much praised for the purity of their atmosphere. It is doubtless unnecessary for us to demonstrate the utility that would attach to such experiments, if they were repeated on a very high mountain, such as Mowna-Roa or Mowna-Kaah in the Sandwich Islands.

Examination of an Anomaly which Atmospheric Temperatures, taken at different elevations, present in the night, when the sky is calm and clear.—The temperature of atmospheric strata diminishes in proportion as these strata become more elevated. There is only one exception to this rule, and that is observed in the night during a calm and clear state of the air. In these circumstances, an increasing progression takes place, to a certain height. According to the experiments of Pictet, to whom we owe the discovery of this anomaly, a thermometer then suspended in the air at two yards from the ground may indicate throughout the night from 3½° to 5⅓° Fahr. *less* than a thermometer similarly suspended in the air, but fifteen or sixteen yards higher.

If it be recollected that solid bodies placed on the surface of the ground, pass by means of radiation under a clear sky, to a temperature much below that of the surrounding air, it will not be denied that this air must at length be affected, by means of contact, with the same coldness, and in a greater degree, according as it is nearer the earth. In this, therefore, we find a plausible explanation of the curious fact made known by the natural philosopher of Geneva. Our navigators will impart to it the character of a demonstration, if they repeat Pictet's experiment in the open sea, by comparing, during a clear and tranquil night, a thermometer placed on the deck with another attached to the mast-head. Not that the superficial stratum of the ocean does not experience the same effects of nocturnal radiation, in the same manner as down, wool, grass, &c.; but after its temperature has diminished, this bed of stratum is precipitated, because its specific density has become greater than that of the inferior liquid beds. We are not, therefore, to expect in this case, the enormous local colds observed by Wells in certain bodies placed on the surface of the earth, nor the anomalous coldness of the inferior air, which seems to be the consequence of them. Every thing, indeed, leads to the belief, that the increasing progression of atmospheric temperature noticed on land, does not exist in the open sea; and that there the thermometer on the deck, and that at the summit of the mast, will indicate very nearly the same degree. The experiment, nevertheless, is not the less deserving of attention. In the estimation of a prudent natural philosopher, there is always an immense distance between the result of a conjecture and that of an observation.

Expeditious Method of determining Mean Temperatures in Equinoctial Countries.—In our climates, the stratum of the earth which undergoes neither diurnal nor annual variations of temperature, is situated at a great distance from the surface of the ground. But such is not the case in equinoctial regions; for, according to the observations of M. Boussingault, nothing more is necessary than merely to sink a thermometer to the depth of ⅓d of a metre (about 1 foot), in order to make it indicate constantly the same degree, or very nearly so. Travellers, therefore, may determine very exactly the mean temperature of all the places they visit between the tropics, either in plains or in mountains, by having the precaution to furnish themselves with a miner's piercer, with which it is easy, in a few minutes, to pierce a hole in the ground of the required depth. It will be found that the action of this instrument on rocks and on the soil, occasions a development of heat, and the observer should always wait till that be entirely dissipated before he commence his experiments. It is likewise necessary that the air in the hole should not be renewed during the whole time of their continuance. A soft substance, such as pasteboard, covered with a large stone, will form a sufficient preventive. The thermometer ought to have a string attached to it, by means of which it may again be drawn up.

The observations of M. Boussingault, of which we have availed ourselves, in order to recommend perforations to the trifling depth of a foot, as conducting very expeditiously to the determination of mean temperatures in all intertropical countries, have been made in sheltered places, in the ground, under Indian huts, and under mere sheds. In these situations, the soil was sheltered from the direct warmth produced by absorption of the solar light, from nocturnal radiation, and infiltration of rains. Every one trying the experiment should place himself in similar circumstances, for there can be no doubt that in the open air, and in places remote from shelter, it would be necessary to penetrate to a much greater depth in the ground, in order to reach the bed possessing an equal temperature.

It is well known that the temperature of the water in wells of moderate depth, also affords an easy and exact mode of ascertaining the mean temperature of the surface. This method, therefore, must not be omitted among those recommended by the Academy.

Observations to be made on Thermal Springs.—If it be the case, as every thing leads us to believe, that the high temperatures of the springs called *thermal*, are solely the consequences of the depth from which they rise, it is natural to suppose that the warmest springs should be the least numerous. At the same time, is it not extraordinary, that none have hitherto been observed whose temperature has approached the boiling point within 36° Fahr.?[*]. If we are not deceived by some vague reports, the Philippine Islands, that of Luçon in particular, are likely to afford the means of elucidating this subject. There especially, as in many other places where thermal springs exist, the most interesting data that can be collected are such as tend to prove that the temperature of a very abundant spring varies, or does not vary, with the lapse of ages; and in particular local observations, with a view to shew the *necessity* of the fluid having a passage across the very deep-lying strata of the earth.

The springs of Aix in Provence, regarded in this point of view, have suggested to me a plan of experiment, of which I think it proper here to insert a notice, as it is very probable that the physical conditions on which it is founded will be met with in other places.

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[*] We do not include in this category of thermal springs the geysers of Iceland, and other analogous phenomena, which evidently depend on volcanoes at present in a state of activity. The warmest thermal spring, properly so called, with which we are acquainted, *Chaudes Aigues* in Auvergne, is 176° Fahrenheit, (+ 80° *centigrade*). Since this article was written for the expedition of *M. Bonite*, M. de Humboldt and Boussingault have given me, as the temperature of the spring *las Trincheras* (Venézuela) in 1800, 195° Fahr. (+ 90°, 4 *cent.*); and in 1823, 206° Fahr. (96° 6 *cent.*) This spring, according to them, has no direct connexion with any active volcano. On the other hand, the Duke of Ragusa writes me, that, at Broussa, at the foot of the Mount Olympus, he found the thermal bath, called by the Turks *Chirurchiest*, to be 183°.2 Fahr. (+ 84° *cent.*) It seems, therefore, that 176° Fahr. (80° *cent.*) is the maximum temperature of European springs only.

The town of Aix, in Provence, possesses baths of thermal water, known under the name of the *Baths of Sextius.* They are surrounded by an edifice, the building of which was completed in 1705. The spring was formerly so copious, that in the last two months of that same year, 1705, it was amply sufficient for the supply of upwards of 1000 baths. The water was amply sufficient for nine pipes of a fountain, and nine stopcocks for baths. From the year 1707 the water began to be less plentiful, and in a few months was so much diminished, that the establishment was wholly abandoned.

Other warm springs exist in this town, at the Cours, in the Garden of the Jacobins, at the Monastery of St Barthelemy, at the Triperie, Grioulet, the Hotel de la Selle d'Or, at the Hotel des Princes, &c., and at the bottom of certain wells, such as that belonging to Sieur Boufillon (in the corner of the *Rüe des Marchands*), and the tanners' pits. These different springs diminished like that of Sextius, and even more rapidly. Many of them, and, among others, the spring of the Jacobins, of St Barthelemy, Triperie, and Grioulet dried up entirely.

While this diminution of the fountains at Aix was going on, to the entire destruction of many of them, some individuals began to turn to their own advantage some very copious springs, which they had discovered, by digging to a small depth in properties situated at a little distance from the town, in the territory of the district called the great and the small *Barret.* The idea that these new waters were just the former waters of the town, soon occurred to the minds of many persons; but the impossibility of proving that such was the fact, for a long time prevented the authorities from interfering. At last, in 1721, during the dreadful plague that prevailed in Provence, Dr Chicoineau of Montpellier, having thought it expedient to order baths for the persons detained in quarantine, Vauvenargues, the commandant of Aix, came to the following resolution : "The warm baths of the town of Aix having appeared to us necessary to wash and purify the convalescent patients ; and as the said baths do not supply sufficient water for this purpose, on account of the quantity that has been withdrawn from the spring by various neighbouring proprietors, we order, for the good of the service, th[illegible] be immediately taken to bring it back, &c. &c." In [illegible]'s order, the consuls caused the holes dug in the district of Barret to be filled up, and *in twenty-two days* after this operation, the waters of the Baths of Sextius were augmented three-fourths, and many springs which had become entirely dry, that of Grioulet, for example, again began to flow.

In May 1772, Vaurenargues having been superseded, the dispossessed proprietors opened, under ground, the work which had been constructed the year before, and immediately the warm springs of the town were seen to diminish, and even entirely dry up.

In July 1722, the breaches were again carefully repaired by the "procureur-general," and the inhabitants of Aix saw the waters reappear. Things continued in this state for five years; but in 1727, the inhabitants of the mills of Barret clandestinely made a new opening in the dam constructed in 1722. The knowledge of this misdeed was only acquired by the falling off in the quantity of water. In order to terminate this obstinate contest between private interest and the general benefit, by a definitive act regarding the right of property, the town caused a stone pyramid to be erected on the lands in 1729.

To these details, which we have entered into in order to establish the fact, that the waters of the pyramid of Barret feed the warm springs of the town of Aix, we shall add, that M. Dauphin, locksmith, assured M. Robert, a doctor of Marseilles, in 1812, that he witnessed an experiment which places the matter beyond a doubt: he stated, that lime was mixed with the water in the basin of the pyramid, and that the springs of Cours and of Mennes became milky.

Under the pyramid of Barret, the water occupies a basin regularly built with stone, about thirteen feet long and upwards of seven feet broad. In June 1812, M. Robert sent down two men to ascertain the temperature of the water; they found it 62° 6 Fahr. (+ 17° cent.). At the same period, the baths of Sextius were at 84° 2 Fahr. (+ 29° cent.)

It appears, therefore, to be established, that the cold waters of Barret become, at least *the greater part*, the warm waters of Aix; while traversing the short space which separates these two points, that is to say, a horizontal distance, which is estimated in the official memoirs, from which we have given an extract, at about a thousand geometrical paces.

It will be observed, that we have employed the words *the* *greater part*, and they in fact indicate precisely the question which remains to be answered. If it could be proved that all the warm water of the baths of Sextius originated from the cold water of the basin of Barret; that the phenomenon does not consist merely of an intermixture which may take place near the surface, between the water of Barret and that of an ordinary thermal spring nearer Aix; that in its passage the fluid does not become chemically charged with any foreign substance, the theory of thermal springs would have made a decided step in its progress. Every one would then be satisfied of their similarity to the Artesian springs, the high temperature of which is evidently to be ascribed to the great depth from which they issue.

Without pretending to devise better means of investigation than the aspect of the places might suggest, I conceive that if permission were obtained to withdraw the waters of Barret, only for a few days, the principal question would be solved. From the time that the thermal spring intermediate between Barret and Aix should begin to flow to Sextius alone, there would be a considerable diminution of the quantity of water, and an increase in the temperature of the baths. A comparative chemical analysis of the respective waters, if performed with that scrupulous accuracy of which we have now many examples, would be attended with much interest. Neither should it be forgotten to repeat the experiment mentioned by the locksmith Dauphin, either employing lime or bran, or some tinctorial matter, were it only for determining the rapidity of the fluid in the subterranean passages which it traverses in passing from Barret to Sextius.

The temporary derivation of the waters of Barret, is the most decisive mode of obtaining the solution of the very ancient problem of physical geography to which thermal springs have given rise; but should this derivation be impossible, there still seems to be a method of attaining this object. The waters of Sextius are said to diminish with drought, and to increase in rainy weather. It is very improbable that the increase and diminution should follow exactly and simultaneously the same relations in the cold, nearly superficial waters of Barret, and those of the thermal fountain nearer the town. If a mixture of the water takes place, we ought, therefore, to expect, that great variations rature would be observed at Sextius.

It may be seen, by this instance, how much the Government has erred in suppressing the office of inspector of thermal waters, under the idea that nothing in that department now remained to be discovered. I now add, in conclusion, that the data on which my plan of experiment is founded, have been derived from a manuscript memoir presented fifteen years ago to the Academy by Dr Robert, which has not, in my opinion, met with that attention which it deserves.

(To be continued.)

Abstract of an Address delivered on presenting the Keith Medal, adjudged by the Council of the Royal Society of Edinburgh to PROFESSOR FORBES, *for his Experiments on the Polarization of Heat.* By Dr HOPE, Vice-President of the Society.*

THE prize founded by our late estimable associate Mr Keith, whose ingenious contrivances for self-registering thermometers and barometers are recorded in our Transactions, is, by the regulation of his Trustees, to be adjudged biennially for the most important discovery communicated to the Royal Society, or in the event of such being wanting, for the best paper which shall have been presented to the Society in the space of two years on a scientific subject. The Council, in discharge of the powers vested in them, have awarded unanimously the Keith prize for the last biennial period, to Professor Forbes, for his paper "On the Refraction and Polarization of Heat," which they consider to come under their class of communications, which contain discoveries important to science.

The Vice-president then observed, that the subject of heat is one so important to man, and so intimately connected with a variety of natural phenomena, that it has not failed to command no small degree of attention in all ages. That an intimate connexion subsists between Heat and Light, and that much discordance of opinion has subsisted respecting the nature of both. He next stated the various opinions entertained concerning them and particularly respecting heat, and in historic order presented the views of Bacon, Boyle, Boerhaave, Stahl, and Black, and adverted to the discoveries of Black respecting latent and specific heat, and the successive labours of Irvine, Crawfurd, Wilke, Magellan, Lavoisier and Laplace, Dulong and Petit, in the same field.

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* The above notice of Professor Forbes' important discoveries, for which the Keith Prize of the Royal Society of Edinburgh has been adjudged, is printed by order of the Council, and is taken from the Report of the "Proceedings of the Royal Society of Edinburgh."

Heat presents itself in two very different conditions; first when combined with matter, pervading bodies slowly, either by communication and conduction through and among its particles, or by the movements of the particles themselves; secondly, when radiated, moving through elastic fluids or empty space with vast velocity.

The first of these had been studied by the philosophers already named, and not long after by Rumford. To the second of these, viz. radiant heat, the subject of Professor Forbes's discovery called upon him more especially to allude, and to present a brief historic view.

The radiation of cold, and its reflection by metallic mirrors, was known to Baptista Porta in the sixteenth century; and observations were made on the radiation of heat, by the Florentine academicians, towards the middle of the seventeenth century, and by Marriotte in 1682. About the middle of the 18th century, Lambert published his works on pyrometry and photometry, which contained some of the first accurate experiments on this subject; and the facts of the difficult transmission and reflection of heat by glass, was pointed out by the Swedish chemist Scheele. Pictet of Geneva extended his experiments on the examination and the reflection of the heat derived from boiling water, and our venerable associate Professor Prevost of the same place, established the doctrine of the mobile equilibrium of heat, in 1802. The triumph of this theory was found in the beautiful experiments of Dr Wells, on dew, in 1813.

Meanwhile, the experiments of Rumford and Leslie were corroborating and extending these general views, even although the doctrines of radiation were denied by the latter philosopher in all his writings. The passage of radiant heat through solid substances, such as glass, and through fluids, such as water, had long been admitted, in the case where light accompanied heat.

But in the case of non-luminous heat, it was strenuously denied by Leslie, and others. The experiments of De la Roche proved that such was the fact, at least in the case of heat derived from terrestrial sources, and at the same time luminous. But this subject has received a vast enlargement by the recent experiments of Melloni, who has shewn that substances differ surprisingly in their permeability to heat, and that while some, such as alum, stop almost every incident ray, others, as rock-salt, transmit almost the whole of the heat, and that from whatever source derived.

The connection of light with heat, was too obvious and important to be overlooked. To Sir W. Herschel the world is indebted for the first great step in this curious inquiry. He examined the thermometric qualities of the spectrum formed from the sun's rays by a common prism of glass; and in 1800 announced the curious fact, that the heating power increases, not only from the violet to the red end of the spectrum, but *even beyond the latter*, indicating the existence of dark calorific rays. These experiments, though at first denied by some authors, were afterwards fully confirmed, and some anomalies which they presented, explained, by Robison, Englefield, Berard, Seebeck, and Melloni.

Heat, then, even unaccompanied by light, appears to be capable both of reflection and refraction. But new modifications of light, discovered of late years, require us to investigate how far the analogy may be pursued. In 1802, Dr Young announced his remarkable discovery of the interference of the rays of light, or the power of two luminous rays, properly disposed, *to produce darkness* by their union. About the year 1808, Malus, a most eminent French philosopher and mathematician, discovered the remarkable modification which light undergoes by reflection from certain substances at certain angles. This modification may be easiest conceived by stating the fact, that light so reflected becomes incapable of undergoing a second reflection in certain positions of the reflecting surface, when common light would be reflected.

The corresponding experiment in the case of heat was tried by Berard, along with Malus, about the year 1811, and an account of them was published in 1817, in the *Memoires d'Arcueil.*

They found, that when the solar beam was twice reflected in the manner just stated, the heat and light refused simultaneously to be reflected in certain positions of the second reflector. The same experiment was repeated with incandescent bodies, with the same result; and even, as stated by Berard, with bodies having temperatures beneath that of visible incandescence. These experiments were probably discontinued in consequence of the death of Malus, and the details were never published, if, indeed, they were ever carried to any great extent. The result has been, that Berard's conclusion seems not to have been generally adopted by the scientific world. The polarization of heat has remained amongst the doubtful facts in science. It has been adopted in scarcely any systematic works, whether British or foreign; and, of late years, direct evidence seemed to be entirely against it. Professor Powell of Oxford, repeatedly and fruitlessly, attempted to obtain Berard's result. Nobili of Florence (whose recent loss science has to deplore) attempted it likewise with the aid of his thermo-multiplier, an instrument admirably adapted for the measurement of small quantities of heat; and Melloni having failed to polarize even luminous heat by tourmalines, concurs in the conclusions of Powell and Nobili. The Vice-President then observed, that it was under these circumstances that the subject was undertaken by Professor Forbes, who, by means of arrangements differing from any that had before been used, has succeeded in completely establishing the polarization of heat under all the circumstances in which light is polarized, namely, by Reflection, Transmission, and Double Refraction, and that it is for the establishment of these facts that the Keith Prize has been awarded by the Council.

Dr Hope then stated that, in the ordinary case of the publication of papers, the Society holds itself in no degree responsible for the truth of the facts stated therein; but, in the adjudication of prizes, the case is different; and that, with regard to them, the Council are bound to be satisfied of the truth of the statements for which they award their prize. Several members of the Council had seen and satisfied themselves of the accuracy of Mr Forbes's leading experiments before the Keith Prize was awarded; and, some days ago, he deemed it right to request Mr Forbes to shew him the more important of these experimental demonstrations. This he succeeded in doing in a way which left upon his mind not the slightest doubt as to the truth of his results; the variations of temperature being so obviously displayed, as to prevent the slightest ambiguity as to the true source from which they are derived. The instrument employed in the research is the thermo-multiplier, of which the invention is due to Nobili, though it has been greatly improved for experimental purposes by Melloni. Professor Forbes has likewise increased greatly its power of indicating the more delicate effects by employing a telescopic apparatus, which enables him to measure a quantity of heat, perhaps not exceeding *one-fifteen hundredth* part of a degree of Fahrenheit.

That the Society may fully understand the nature of the proofs afforded by Mr Forbes's experiments, reference must be made to the correlative facts observed in the case of light.

When light undergoes reflection from glass at an angle of 56°, its physical character is found to be thus far altered, that it refuses to be a second time reflected by another plate of glass placed to receive the ray at the same angle of 56°, if the plane of incidence on the second glass be perpendicular to the plane of incidence on the first. The light is then wholly transmitted by the second plate. If the plane of incidence be the same for the two plates, complete reflection takes place at the second plate. This illustrates polarization by *reflection*.

If a number of glass plates be used, and light *transmitted* obliquely through such a bundle of plates, it is in like manner found, that the emergent light is wholly transmitted by a second similar bundle placed parallel to the first, but is almost wholly reflected, and therefore *not* transmitted, when the second bundle is placed so that whilst the ray falls upon it at the same angle as upon the first, the plane of incidence on the second bundle being perpendicular to the plane of incidence upon the first bundle. This is polarization by *transmission* or *refraction*.

Lastly, It was observed before the close of the 17th century by Huyghens, that certain bodies, as Iceland spar, endowed with the property of double refraction, alter at the same time the character of the light in the two refracted rays. So that, if two sections similarly cut from a crystal of Iceland spar be placed upon one another in *conformable* positions, or the respective positions which they occupied on the crystal, the two rays will proceed through the second slice as they did through the first, and be refracted according to the same laws. But if the second slice be placed *unconformably* upon the first, or turned round a quarter of a circle, the ray, which at first was ordinarily refracted, is now extraordinarily refracted; and the ray, which at first was extraordinarily, is now ordinarily refracted. Now, it has been found that some crystals, such as tourmaline, possess the property, first, of dividing these rays, *and then of suppressing or absorbing one of them*; the result of which is, that when two tourmalines, cut as we have supposed, are placed *conformably*, the ray which was not suppressed by the first slice, still makes its way through the second; but, when placed *unconformably*, the ray transmitted by the first plate is wholly suppressed by the second. In the latter case, therefore, not a ray of light can penetrate the two plates. This is polarization produced by *double refraction*.

Now, all these modes of polarization have been recognised by Mr Forbes in the case of heat, and even in the case of heat wholly unaccompanied by light. The Vice-President announced that he had witnessed this in the most satisfactory manner in the case of heat polarized by reflection and transmission, for which purposes, instead of glass, (which permits scarcely any non-luminous heat to penetrate it), Mr Forbes employs plates of mica, divided by a peculiar process into extremely thin laminæ.

But the analogies which he has established between light and heat do not stop here. It has been found in the case of light, that, when the two reflecting plates before spoken of, or the two crystals, are placed in *unconformable* positions, so that little or no light reaches the eye, we may, by interposing between the plates or the crystals a thin lamina of a doubly refracting substance (such as mica) in a certain position (relatively to its internal structure), cause a portion of light, which before was incapable of reaching the eye, to become capable of so doing. In other words, the polarized light, which at first was incapable of reflection or transmission at the second plate or crystal, now becomes capable of it; it has lost, to a certain extent, its character of polarization, or it is said to be depolarized.

Dr Hope stated, that he had seen this to be most completely effected in the case of heat, by Mr Forbes. A lamina of mica is interposed between the bodies used to polarize heat unconformably placed. When the lamina of mica has a certain position, no effect is produced beyond stopping a small portion of the heat, which would otherwise reach the thermometer; but when this interposed lamina is turned 45° in its own plane, a portion of the heat which before was incapable of reaching the thermometer in consequence of its polarization, is now capable of doing so, and the influx of heat is instantly indicated. The most striking exemplification of this result is found in the fact, which excited so much interest when communicated more than a year ago to the Society, that in certain cases the mere interposition of a piece of mica (in the proper situation), will cause an immediate indication of increased temperature, the mica *depolarizing* more heat than it *stops*. Since depolarization takes place only in consequence of double refraction, we have here another undoubted proof of the double refraction of heat.

The Vice-President terminated his general and rapid sketch, in which he alluded to the brilliant discoveries of Brewster, Arago, and Fresnel, respecting the polarization of light, by observing, that it would be needless for him to point out the important bearing of these facts on the question of the nature of heat, and its connection with light. He concluded in the following terms:—"It now only remains for me to present to Professor Forbes the medal which has been awarded to him for these discoveries. I believe that I shall be joined cordially by every member of the Society who now hears me, in the fervent wish that it may be the will of the Almighty Ruler, that his life may be long protracted, with vigour of mind and health of body to pursue the career in which he has made an advancement so honourable to himself, and reflecting lustre upon those great establishments, the University and the Royal Society, with which he is connected. I cannot doubt that he will persevere in this happy path with the same ardour and success which have hitherto accompanied his researches. Indeed, we have a gratifying proof that his zeal will not be impaired, nor his success less brilliant, from the discovery in the same field announced by him at the last meeting of the Society, of the Circular Polarization of Heat."

Description of several New or Rare Plants which have lately Flowered in the Neighbourhood of Edinburgh, chiefly in the Royal Botanic Garden. By Dr GRAHAM, Prof. of Botany.

March 10. 1836.

Poinsettia.

Involucrum monophyllum, androgynum, basi 5-loculare, extus appendiculatum, nectariferum. Flores pedicellati, nudi; masculi bifariam in singulis loculis ordinati, monandri; fœminei solitarii, germen trilobum, ovulum solitarium singulis lobis.

Poinsettia pulcherrima.
Euphorbia pulcherrima, Herb. *Willd.*
Euphorbia Poinsettiana, *Buist* MS.

DESCRIPTION.—*Shrub* erect, ramous; branches round, young shoots bluntly 4-angled, green, glabrous, hollow. *Leaves* scattered, occasionally opposite, spreading, petiolate, ovato-elliptical, subacute, sinuated, veined, soft and pubescent on both sides, bright green above, paler below. *Petioles* furrowed above. *Bracteæ* similar in shape to the leaves, but aggregated at the extremities of the branches, and of splendid vermilion colour, paler below. *Cymes* terminal, subtrifid. *Involucres* green, on short stout erect footstalks articulated at the base, ovato-orbicular, toothed, marked by five sutures on the outside, with which alternate on the inside five falcate processes, beginning with narrow extremities at the mouth of the involucre, and, adhering to this with their backs, become gradually broader below, passing inwards, and attached to an elevation in the centre, divide the lower part of the involucre into five distinct cells, and supporting on their edges erect fimbriæ, they divide the upper part also, but less completely; teeth of the involucre numerous, coloured like the bracteæ, woolly on the inner side, fringed at their extremities, connivent. *Appendage* single on the outside of the involucre towards the axis of the cyme, round, entire, peltate, folded in the middle, so as appear 2-lipped, nectariferous. *Male Flowers* about 14, in two rows in each loculament and arising from its base, erect, petiolate, naked, monandrous, mixed with chaffs (abortive male flowers?) which are woolly at the apex, and occasionally tinged red there; *petiole* colourless, as long as the involucre; *filament* red, at length hanging over the edge of the involucre; *anthers* 2-lobed, lobes divaricated, so that those which are next to each other in the two rows of stamens overlap, opening along their outer sides; *pollen* yellow, granules round. *Female Flower* solitary, central, petiolate, naked; *germen* 3-lobed, each lobe emarginate; *style* awanting; *ovule* solitary in each lobe. These appearances I describe as I saw them, but the female flowers were probably imperfect, none enlarged, projected beyond the involucre, or produced seed; but after a while, a small number of the male flowers in succession having been perfected and protruded beyond the involucre, this became yellow, and the whole separated at the articulation near the base of the footstalk.

By whom this truly splendid plant was communicated to Willdenow's Herbarium, I am not informed; but it was again discovered in Mexico by Mr Poinsette, and sent by him to Charleston in 1828, and afterwards to Mr Buist of Philadelphia, who has within a very few years brought together a choice collection of plants, equally creditable to his enterprize, and promising as a point from which will be diffused a greater knowledge of the vegetation of North America. From Mr Buist it was brought by Mr James Macnab to the Botanic Garden, Edinburgh, and to several other establishments in this country, in November. 1834: from the information communicated by him, it has since been imported into other

British collections from Mr Buist's garden. It flowered twice with us last year, but too imperfectly to allow of its being figured. It subsequently flowered with Dr Neill at Canonmills, and again with us this month (February 1836). Nothing can be more ornamental in the stove. The rose-like whorls of bracteæ which terminate the branches, have been seen on the large plants cultivated at Philadelphia, as much as twenty inches across, and equal in colour to the finest tints of *Hibiscus Rosa-sinensis.* There can be no doubt that it forms a new generic type, though in several species of *Euphorbia*, especially *E. splendens*, there are the rudiments of the remarkable septa found in the involucre here. I have dedicated it, if not to its original discoverer, at least to one who has first brought it into cultivation and into general notice among botanists, and from whose exertions many additions to our collections of plants from Mexico are expected. At Philadelphia the plant is exposed in the open air during summer, but is placed in the stove during winter, at which season, or early in spring, there, as here, it seems to have its period of flowering.

Sceptranthes.

Tubus clavatus erectus; limbus suberectus. Filamenta tubo adhærentia, alternatim breviora; antheræ lineares, erectæ, prope faucem tubi subsessile. Stigma trifidum erectum. Germen stipitatum.

Sceptranthes Drummondii.

Zephyranthes Drummondii, *Don!* in Sweet's Brit. Fl. Gard. 328.

Description.—*Bulb* about the size of a walnut, spheroidal, covered with a brown unbroken skin, terminated with erect oblong segments in several layers at the top of the elongated cylindrical transversely wrinkled neck of the bulb. *Leaves* six in the specimen described, two in one and four in another bulb in the Botanic Garden, neither of which have flowered, of unequal length (the longest two, the shortest one foot long, 4–5 lines broad) linear, broadly channelled above, blunt, pruinose. *Scape* (to the base of the spathe 7 inches long) lateral, erect, hollow, reddish-yellow at the base, becoming gradually greener upwards. *Spathe* (1½ inch long) membranous, ribbed, perforated, and abruptly marcescent near the apex. *Perianth* erect (2 inches long, 1½ inch across) tube cylindrical, greenish-yellow; limb white, 6-parted, segments obovate, attenuated and parallel, and in contact in their lower half, subspreading above, ribbed, the three outer rather the largest, and, more distinctly than the inner, terminated by a greenish mucro. *Stamens* 6; filaments incorporated with the tube of the perianth; anthers linear, situated near the mouth of the tube, three alternate ones a little lower down; pollen yellow, granules oblong, and somewhat angled. *Germen* stipitate, footstalk as long as the more persisting part of the spathe. *Style* little more than half the length of the tube, filiform, pale green. *Stigma* 3-fid, the segments short, nearly erect.

Bulbs of this very pretty plant were sent from Texas by Mr Drummond, and distributed to various botanical establishments in Scotland in the beginning of 1835; but I am not aware of their having flowered any where excepting in the collection of Dr Neill last autumn, and in the nursery garden of Messrs J. Dickson and Sons, where, in the stove, the specimen described expanded a handsome flower in the beginning of March 1836.

The length of tube, and especially the adhering filaments, seem to me to remove the plant from the genus *Zephyranthes;* the greater shortness of the tube, the less flattened limb, and the stipitate germen, prevent me from uniting it to the genus *Cooperia.*

All the discoveries of one admirable collector—whose untimely death we shall never cease to regret—have not yet been made known, when we have received accounts, I fear in too authentic a shape to be doubted, that another has fallen a sacrifice to his exertions in behalf of Botany. The kind-hearted conduct, and cheerful conciliatory behaviour of Mr Richard Cunningham, Colonial Botanist in New South Wales, has not been able to protect him from the ferocious hostility of excited savages, and, we have reason to believe, I fear almost from official authority, that he has suffered a violent death when with an exploring party in the interior of New Holland;—a party which we know by letters from himself he joined with the warmest anticipations of contributing largely to our knowledge of Australian vegetation. Within two years, Douglas has been lost by an attack from a wild bull—Drummond, we believe, from climate—and Cunningham, from what is worse than both, and less under restraint than either, the madness of his fellow men.

Proceedings of the Royal Society of Edinburgh.

1825, *December* 7.—Dr Hope, V. P. in the Chair. The following communications were read:—

1. On the Poisonous Properties of Hemlock, and its lately discovered alkaloid, Conia. By Dr Christison.

The author commenced by stating, that he had repeated the greater part of the analysis of hemlock lately executed by Professor Geiger of Heidelberg, and had obtained precisely the same results. According to his analysis, hemlock contains a peculiar principle, alkaloidal in its nature, but differing from the previously discovered alkaloids in its form, which is that of an oily-like liquid, volatile at a moderate elevation of temperature, and capable of being readily distilled over with water. It neutralizes acids, without however forming crystallizable salts. It contains a considerable proportion of azote. It quickly undergoes decomposition when exposed to the air, giving out ammonia, and becoming a dark, resinous-like substance.

The discoverer inferred, from a few experiments chiefly made on birds, that this principle, which may be termed *Conia*, from the genus of plant whence it is obtained, possesses active poisonous properties; that it produces coma, convulsions, and depressed action or even paralysis of the heart; and that its poisonous qualities are greatly impaired by combination with acids. The author, however, has been led to conclude, from an extensive set of experiments on the higher orders of animals,—that the effects of Conia on the body are increased rather than diminished by neutralization with an acid, such as the muriatic; that it does not produce coma when administered either free or combined; that it does not act at all on the heart; that it possesses a local irritant action, and that its remote action consists simply in the production of swiftly increasing paralysis of the muscles, ending fatally by asphyxia from palsy of the muscles of respiration. He farther found it to be a poison of exceeding activity, scarcely inferior indeed in that respect to hydrocyanic acid. Two drops applied to a wound, or introduced into the eye of a dog, rabbit, or cat, will sometimes occasion death in ninety seconds; and the same quantity injected in the form of muriate into the femoral vein of a dog killed it in three seconds at farthest. The author added various reasons for doubting the probability of any chemical antidote being discovered; and suggested artificial respiration as the most probable remedy, founding on an experiment in which the heart was maintained in a state of vigorous action for a long time by artificially inflating the lungs.

An abstract was then given of a set of comparative experiments made with extract of hemlock; from which he inferred that the action of hemlock is identical with that of Conia. Very powerful extracts were used, which had been prepared with absolute alcohol from the leaves or seeds. The effects ascribed by some toxicological authors to hemlock were not observed; but simply paralysis, with intermittent slight convulsions. From this identity of action it may be concluded, that Conia is really the active principle of hemlock, or at least contains it in large quantity, and is not the product of chemical action and new arrangements of elements.

Some remarks were appended as to the probable nature of the State-poison used in ancient times, particularly in Athens, for despatching criminals; which has commonly been held to be a preparation of the same plant with the modern *Conium maculatum.* The author shewed, from the descriptions of the Greek *κωνειον* and Roman *cicuta,* that this plant could not be the modern *conium*; that the account given by Plato of the effects of the state-poison in the case of Socrates is wholly at variance with the description by Nicander and others of the action of the *κωνειον*; that the effects ascribed to the poison in Plato's narrative are such as no poison whatever which is known at present can produce; and that consequently either Plato's description is an embellished narrative, or the ancients were familiar with a poison of most remarkable and peculiar properties, with which modern toxicologists are no longer acquainted.

2. The reading of a paper on the Geology of Auvergne, by Professor Forbes, was commenced.

December 21.—Dr Hope, V. P. in the Chair. The following communications were read:—

1. Notes on the Geology of Auvergne, particularly in connection with the Origin of Trap-Rocks and the Elevation Theory. By Professor Forbes. Concluded.

The *first* part of this paper (which accompanied a series of geological specimens from Auvergne, presented to the Society) relates to several specific points which tend to assimilate the evidence for the igneous origin of trap-rocks generally, with that afforded by the volcanic district of Central France. The altered character of the stratified deposits with which igneous rocks have been intermixed, is one of their most striking features; yet we occasionally find cases where this evidence is far from being so obvious as might be expected; and this dubious character, which is particularly remarked in the hill of Gergovia, near Clermont, forms an admirable parallel to some cases in trap districts where a like want of alteration occurs.

The mineral character of the rocks of Auvergne admits of almost perfect identification in a majority of cases with that of undoubted trap-rocks; and we may employ the formations of Central France as a medium of comparison between trap-rocks generally, and modern volcanos, from which the formations of the Mont-Dôme are undistinguishable. The trachytes of the Mont Dor and Cantal find their counterparts in the districts of the Siebengebirge and Laacher-See. Various points of structure were noticed as important, especially the columnar forms of lavas, geologically speaking, modern, which has been often referred to; and more remarkably the union of the tabular, with the polygonal columnar structure, exhibited in the undoubtedly igneous trachytes, basalts, and phonolites of the Mont Dor, which are sometimes so extensively slaty as almost to assume the appearance of stratified rocks. The very remarkable passage of one rock into another differing in mineral character and structure was also pointed out, and hence the difficulty of pronouncing conclusively upon the relative age of such rocks.

The *second* part of the paper referred to Von Buch's Theory of Elevation-Craters, and professed to give simply the impression made upon the author's mind by an examination of the specific cases of the groups of the Cantal and Mont Dor, which have been quoted as examples in support of that theory. Various views of the subject were presented, from which the author is disposed to conclude decidedly in favour of the Elevation Theory in these particular cases. The arguments were drawn chiefly from the forms and magnitude of the valleys, and the relation of the beds of igneous rock to one another, in which the valleys are formed. The author expresses some doubt as to the utility of the calculations entered into with regard to this question by MM. Elie de Beaumont and Dufrenoy, and especially as regards the complicated system of the Mont Dor, of which he considers it almost hopeless to unravel the manifold revolutions. In general, however, he coincides in the conclusions arrived at by those authors.

2. Notice of a New Compound of Sulphur, which is probably a Sulphuret of Nitrogen. By Dr Gregory.

3. On another New Compound of Sulphur, analogous to the Mercaptan of Zeise. By the same.

4. On a curious Phenomenon observed in the Island of Cephalonia, and on the proximate cause of Earthquakes in the Ionian Islands. By Dr John Davy.

1836, *January* 4.—Sir THOMAS M. BRISBANE, President, in the Chair. The following communication was read:—

Some Observations on Atmospherical Electricity. By Dr John Davy, F. R. S.

January 18.—Right Hon. Lord GREENOCK, V. P. in the Chair. The reading of the following paper was commenced:—

1. Observations and Experiments on the coloured and colourable matters in the Leaves and Flowers of Plants, particularly in reference to the Principles upon which Acids and Alkalies act in producing Red and Yellow or Green colours. By Dr Hope.

Proceedings of the Wernerian Natural History Society. (Continued from p. 201.)

1835, *Dec.* 19.—Professor TRAILL, V. P. in the Chair. Dr Martin Barry demonstrated the Ganglion oticum in the human subject, as dissected by himself, under the direction of Professor Tiedeman of Heidelberg.

Professor Jameson read Mr Dufrenoy's account of the period and mode of formation of the Monte Somma, or exterior of Vesuvius, and of Mount Vesuvius itself, shewing that the latter probably did not exist before the great eruption of the year 79. He also communicated a note respecting the fossil elk of Ireland and Isle of Man, shewing that we possess no historical evidence of the animal being known as a living species; the rude figure in the Cosmographia of Munster not representing the elk but the fallow deer.

Sir Patrick Walker exhibited some insects which prove very destructive to the pine forests in the Highlands of Scotland, and made a few observations on their mode of boring into the wood.

1836, *Jan.* 9.—Professor TRAILL, V. P. in the Chair. Mr James Wilson read a paper on the birds included under the genus Eurylaimus of Horsfield, illustrating his remarks by specimens and figures.

Dr Deuchar gave an account of some new tests for easily distinguishing Carbonates from Bi-Carbonates, and exhibited the mode of making the experiments.

Sir Patrick Walker then read notices regarding the occurrence, near Edinburgh, of several native birds, generally regarded as extremely rare; particularly the *Motacilla neglecta*, first remarked by him on the banks of the Water of Leith in 1804 (but referred by him to the *Motacilla flava*, until he became acquainted with Gould's observations), and often observed since that time; likewise the Redstart, *Sylvia Phœnicurus*, in various places around the city; the Dusky Grebe, shot at Lochend; and the *Ardea minuta*, killed at the mouth of the Tyne in East Lothian.

The members then removed to the portico, to witness an experiment, performed in the open air, by Mr K. T. Kemp, shewing the solidification of sulphurous acid.

—— *Jan.* 23.—Bindon Blood, Esq. V.P. in the Chair. A paper was read entitled, Remarks on the circumstances to be chiefly attended to in the execution of a Geological Survey of Scotland.

Mr Kemp then shewed a method of liquefying Chlorine at a cold of — 26° Fahr.; and of keeping it in a liquid state, at the temperature of the atmosphere, by a pressure equal to five atmospheres and a half.

1836, *Feb.* 6.—Robert Stevenson, Esq. V. P. in the Chair. Mr Kemp described and exhibited experiments, proving that chlorine, iodine, bromine, &c. bleach without the decomposition of water or the presence of oxygen gas. He likewise shewed a new modification of the differential thermometer, capable, according to Mr Kemp, of ascertaining whether the moon's rays, when concentrated, possesses heat equal to — 100° Fahr.

Professor Jameson communicated Suggestions by the Reverend Mr Robertson of Inverkeithing, of easy methods of analysis, for practical purposes, of the mineral waters usually mét with.

Dr Martin Barry laid upon the table some specimens of Red Sandstone, from the county of Tyrone, abounding in fossil fishes of the palæoniscus tribe. Some discussion took place as to the age of the sandstone.

Dr Traill exhibited specimens of Sandstone-flag, containing large scales of fishes, having a particularly marked surface, from Pomona, Orkney.

The Society, on the suggestion of the President and other members of Council, recommend an application to the Light-House Board for the cutting of marks on rocks at half tide-level, and the superintendence of these by the Lighthouse officers, with the view of ascertaining whether the land of Scotland is stationary, or is rising above or sinking under the present half-tide level. Mr Christie of Banff communicated an account and specimen of a species of the Ammonite family, found in the lias of Banff, a deposit first discovered in that part of Scotland by Mr Christie.—At the same meeting, Mr Champion of the 91st regiment communicated observations on the phenomenon, noticed by Dr Davy in the island of Cephalonia; and the proposal for a Zoological Garden at Edinburgh was strongly recommended by the Society.—Professor Jameson exhibited a series of birds, collected by Captain Clunie, New South Wales, among which were specimens of the Sula alba from Moreton Bay.—A new species of Pernis, from India, which was named Elliotii, in honour of Mr Elliot, an intelligent observer by whom the specimens were brought home and presented to the Museum. Two Buzzards, lately killed in Britain, were exhibited; one of these very nearly allied to the Falco Jackall of Le Vaillant, was killed near Birmingham; of the other, killed near Newcastle, a minute description was communicated by Mr William Jameson.

—— *Feb.* 20.—ROBERT JAMESON, Esq. P. in the Chair. Professor Forbes read Remarks on the Physical Geography of the Pyrenean Range, chiefly in connection with the celebrated hot springs of that district; and exhibited an extensive collection of specimens of the rocks and minerals of the Pyrenees. The Professor at the same time presented his collection of Pyrenean Rocks to the Royal Museum of the University.

Mr Kemp exhibited some experiments which he considered as shewing that, on ignition, by galvanic electricity, carbon is volatilized in the Torricellian vacuum.

Proceedings of the Society for the Encouragement of the Useful Arts in Scotland.

THE Society for the Encouragement of the Useful Arts met in the Royal Institution, on Wednesday the 13th January 1836, at 8 o'clock, P. M., EDWARD SANG, V. P. in the Chair.

The following communications were laid before the Society:—

1. Supplementary Description and Drawing of certain Additions to the Turning-Lathe for facilitating slow turning. By Mr James Whitelaw, 18 Russell Street, Glasgow.

2. Letter from J. Stewart Hepburn, Esq. of Colquhalzie, as to simpler modes of working the Valves in his Air Extractor for Syphons, than that formerly proposed by him.

3. A Model of a New Escapement, by Mr Duncan Macgregor, Comrie, was delayed till a full description be sent.

4. Supplement to his Communications of 12th November 1833, on an Instrument for Cutting Coats, with a Drawing, by Mr William Smith, Cupar-Fife.

In reference to this subject, Mr Macdonald, West Register Street, laid before the meeting the result of a great number of measurements taken by means of his Andrometer, which clearly indicated the necessity for measurements more numerous than those proposed by Mr Smith.

5. A Report to the Lighthouse Commissioners, by Mr Alan Stevenson, presented by him to the Society, was then read.

In this paper Mr Stevenson contrasted the advantages and disadvantages of the new mode of lighting adopted at Inchkeith. Taking the increased brilliancy and the additional expense into account, he found the balance in favour of the new method. The Society seemed highly pleased with the paper, and some of the members took notice of the great merit which Mr Stevenson had in overcoming his previous opinion on the subject of dioptric lights. John Robison, Esq. incidentally noticed the progress which is being made in the manufacture of the large lenses for the May Light. It did not appear in the report, whether Mr Stevenson had taken into account the additional number of rays of light. The old apparatus gave light in four directions at once—the new gives light in seven, and thus a greater number of vessels have the use of it at once; if this has not been allowed for, the advantage of the new light will have to be augmented in the ratio of 7 : 4.

The following candidates were admitted ordinary members:—

1. Thomas Stewart Traill, Esq. Esq. M.D. F.R.S.E., Professor of Medical Jurisprudence in the University of Edinburgh, 10 Albyn Place; 2. George Lees, Esq. A.M., Lecturer on Natural Philosophy, 8 Regent Terrace; 3. Mr James Slight, Engineer, Panmure Place, residing in Reid's Court, Canongate; The Rev. J. P. Nichol, 17 Archibald Place; Mr Peter Wilson Roy, Music Seller, 'lermont Street West.

Jan. 27.—Thomas Grainger, Esq. in the Chair. The following communications were laid before the Society :—

1. Part third of a paper, on the Construction of Oblique Arches. By Edward Sang, Esq. teacher of Mathematics, Vice-Pres. Soc. Arts. Drawings were exhibited.

In this part of the paper the forms of the arch-stones were examined, and the methods of delineating and modelling them described. The waste of material on account of the rhomboidal forms of the stones was also discussed, and it was shewn, that the principal loss occurs on the ends of the stones, while scarcely any waste is occasioned by the twist. It was also mentioned, that any waste occasioned by twist must have arisen from the use of improper lines. Mr Sang stated, that he had revised and extended the former parts of the paper; and that, on investigating the appearances of the joints when viewed from different sides, he had found the end view to present the appearance of the Tractory, a curve well known to students of the higher geometry; while the side view of it is a new curve, the companion to the tractory, closely allied to the tractory and catenary, and capable of being described by a slight modification of Leslie's apparatus.

2. Description and Drawing of a New Escapement. By Mr Duncan Macgregor, Comrie.—A model was exhibited.

3. Description of a New Construction of, and mode of propelling Steam-Boats. By Dr A. Plantou, Philadelphia, United States, the American patentee. Communicated by the Royal Society of Edinburgh.

4. A large accurate model (about seven feet long) of the celebrated American Steam Raft-Boat, which plied for some time on the River Hudson;—the machinery of which was fitted up by Mr Neil Snodgrass, now engineer, Glasgow;—was exhibited through the kindness of Mr Snodgrass. An explanatory letter from that gentleman was read.

5. Donation.—Solution of Algebraic Equations of all orders, whether involving one or more unknown Quantities. By Edward Sang, Esq. Teacher of Mathematics, Edinburgh. Edin. 1829. From the Author. Laid on the table.

6. The report of the Committee on Mr M'Pherson's New Pinion Ball-Cock, was read and approved of.

7. A letter from Arthur Aitken, Esq. Sec. of Arts, London, was read, acknowledging receipt of the Select Printed Papers of the

Society, recently forwarded to the London Society of Arts; and returning thanks for the donation.

The following candidates were admitted as Ordinary Members, viz.

1. Mr James Haldane, Brass-founder (of Haldane and Rae), 5 Physic Gardens; 2. Mr Charles Cowan, Paper-maker, Valleyfield, Penicuik; 3. Mr Charles Lawson, Seedsman, 8 Hunter Square.

Feb. 17.—Edward Sang, Esq. V. P. in the Chair. The following communications were laid before the Society:—

1. Model, Drawing, and Description of a new Nose-pipe for Fire-engines; having a contrivance by which, in a moment, the water may be thrown either in a column, as in the common jet, or spread out in a sheet. By Mr Adam Hope, Jedburgh.

The change from one kind of jet to the other is effected by means of a cylinder, through which two apertures are cut, the one with a round, the other with an elongated opening. The cylinder turns in its place, so that either one or other opening can be used. It was stated, however, that as, during the change, both apertures are for a short time closed, the sudden stoppage of the current would rend the pipes, while, by merely placing the thumb over the opening of the common jet, the same effect can be obtained.

2. Embossed Maps for the Blind; and specimens of printed Music for their use, upon a new principle of notation, applicable to music in general. By Mr James Gall, jun. 24. Niddry Street, Edinburgh.

The embossed maps, formed by pressing thick paper into cavities prepared in a piece of metal, exhibited the outlines of countries, the courses of the rivers, and the ridges of mountains very distinctly, and seemed to possess the strength requisite for insuring their permanence. The principle of the new notation of music is to employ numbers referring to the key-note of the tune instead of the five bars and the characters now in use. Mr Gall developed his ideas on this subject at considerable length, and laid great stress on the saving of room which would result from the adoption of his system, as well as on its greater perspicuity.

3. Description of a new Process of Engraving in Relief on Copper, called Metallic Ectypography. Invented by A. Dembour, engraver and lithographer, Metz. With Plates. Printed at Metz, 1835. Communicated by John Robison, Esq. Couns. Soc. Arts.

4. A new mode of Heating a Bath, by means of a portable boiler.

By Mr John Macpherson (of Smith's Heirs), Blair Street, and Mr C. H. Smith, garden architect, Edinburgh.

The Bath was heated in the rooms during the meeting, and the following is the result of the trial, viz. the temperature of the water in the bath before the fire was lighted was 51° Fahrenheit, and after the water had been applied for forty-six minutes, the temperature of the water was risen to 110°.

5. The Report of the Committee on Mr Edgar's Wooden Bridge was read and approved of.

The following candidates were admitted ordinary members:—

1. Thomas Greig, Esq. 5 Bucclеugh Place; 2. James Hunter, Esq. M. D. 2 Cassells' Place; 3. Grant S. Dalrymple, Esq. 19 Broughton Place: 4. Mr James Milne, Brassfounder, Chalmers' Close, 39 Lauriston Place; 5. Mr Alexander Bryson, Watchmaker, 8 South Bridge Street.

SCIENTIFIC INTELLIGENCE.

1. *On the supposed Existence of a New Small Planet.* By M. Cacciatore, Director of the Observatory at Palermo.—On the 15th February, M. Arago read to the Academy of Sciences, the following extract from a letter communicated to him by Captain Hall, and which had been addressed by M. Cacciatore to Captain Smyth, "I have something important to communicate to you. During the month of May 1835, while I pursued observations, with which I have for a long time been occupied, on the proper movements of stars, I saw, near the seventeenth star of the twelfth hour of the catalogue of Piazzi, another star, which seemed to be also of the seventh or eighth magnitude; I noted the distance which separated them. The weather did not permit me to observe during the two following nights. It was only on the third that I again saw the new star; it had moved a good deal towards the east and towards the equator; clouds forced me to delay my observations for another night; but from that time till the end of the month of May, the weather was dreadful; the winter seemed to have recommenced at Palermo: heavy rains, and violent winds succeeded, and to such an extent, as to prevent all kinds of researches. Fifteen days afterwards, when I was again able to proceed with my observations, the star was immersed in the twilight of the evening, and all my efforts to find it were fruitless; the stars of that magnitude were no longer visible. The estimated movement, in three days, seemed to me 10″ in right ascension, and about one minute (or a very little less) in declination, towards the north. A movement so slow, induces me to suppose that the star is situated beyond

Uranus. I felt great disappointment at not being able to follow up an investigation so important." On this subject *M. Arago* makes the following observations:—"There is in this communication a circumstance which astronomers will have much difficulty in understanding. M. Cacciatore says, that when the weather became favourable at Palermo, towards the end of May, the moving star was no longer visible, owing to the crepuscular light of the evening. This explanation is admissible when the question regards the passage of the star to the meridian; but two or three hours after sunset, or at night, nothing could prevent the comparison of the suspected planet with the neighbouring stars, either by means of a parallactic machine, or with the great azimuth circle, which holds the first rank among the instruments of the observatory at Palermo. It seems to us inconceivable that an observer so meritorious as M. Cacciatore, opposed by unfavourable circumstances though he was, should not have been able to confirm the truth of such a capital discovery,—that he should not have judged it proper to follow the star beyond the meridian.

2. *Climate of Palestine.*—In the Annuaire of 1834, M. Arago published a memoir, which had for its object to prove that, since the time of Moses, the temperature of Palestine has undergone no sensible alteration. The Duke of Ragusa denies the accuracy of the facts on which the conclusion is founded. He says, " There are now no palms in the part of Palestine indicated by the memoir." But, nevertheless, I find farther on in the Marshal's communication, " that there are a few at Jericho;" that at Jerusalem he saw three " nearly barren;" at Rama, a place cited in the article in question, " there are some which yielded fruit:" but certainly if there are some at that spot, a great many might exist. One single palm-tree producing ripe fruit, would be sufficient in a question as to the temperature. The limit assigned, in the same article of the Annuaire, to the cultivation of the vine, is also called in question. We here transcribe this portion of the memoir, in order that botanists themselves may decide if the facts adduced by the Duke of Ragusa are of a nature to modify their old opinions. " The article fixes at between 21° and 22° cent. (69°.8 and 71°.6 Fah.) the maximum of temperature that the vine can bear when productive, and, to justify this assertion, it states, that at Cairo, where the mean temperature is 71°.6 Fahr., the vine is not cultivated on the great scale, and that there are there only detached vine plants. This is the fact in regard to the past, but then the cause is quite of another description. Considerable plantations of vines have lately been made, which promise to afford excellent returns; but a decisive fact is, that there have always been, and still are, vines in Fayoum, which is one of the hottest provinces in Egypt owing to the hills of sand which surround it on all sides. These vines are situated at the villages of Fidemia, Adjamira, and Tumban; they are cultivated by the Cophts, and yield agreeable wines. That which I have drunk presents a phenomenon which is rare in such a climate; it does not affect the head, and is drinkable after the

second year. Pocoke, who travelled in 1737, speaks of the cultivation of the vine by the Cophts in Fayoum, and, what is still more important, there is in the higher part of Upper Egypt, at Esné, twelve leagues to the south of Thebes, a vineyard which has an extent of several feddams. Its original object was doubtless to yield grapes for eating, but Jussuff Kiacheff, formerly soldier in the army sent to Egypt, and who was taken prisoner by the Mamelukes at the period of the evacuation, and remained in the east, informed me that he farmed the vineyard; that he made excellent wine of the produce, and obtained a quantity equal to that afforded by the vineyards of Europe. We may then conclude from these facts, that if in Egypt, till within a few years, the vine has not been cultivated on a great scale, it is because the inhabitants do not drink wine, and that we are not to draw the inference, that there is a maximum of temperature above which the vine does not yield the means of making wine."

3. *Indications of a Change in the relative Levels of the Land and Sea on the West Coast of Scotland.*—Mr Smith of Jordanhill is at present engaged in the investigation of this subject, and has found at Glasgow, between the diluvium and the recent beds of sand formed by the river, a marine deposit of finely laminated clay containing marine shells, all of them identical with recent species, except a natica. This deposit has been met with from a few feet to *seventy feet* above the present level of the sea, and besides shells, contains sea-weeds and bones of fish and sea-fowl. It would, says Mr Smith, be termed a recent pliocene formation by Mr Lyell. The shells seventy feet above the level of the sea, were found on the banks of Loch-Lomond;* and Mr S. has observed near Glasgow, shells at the height of fifty-five feet contained in clay, which must originally have been found at the bottom of the sea, and is now sixty feet above high water-mark.

4. *Facts relating to the Soulevement or Rising of Scandinavia at a recent epoch.* By M. Keilhau.—In the remarks I am about to offer, it is not my intention to speak of the remarkable rising taking place at the present time in a part of the districts situated near the Baltic, but rather to treat of the risings which, at a period more or less remote, have occurred at certain intervals, and may be compared to great shocks; and whose traces are so analogous to the facts which have been observed particularly in Chili immediately after the earthquake of 1822, that we are at once induced to place them in connexion with the earthquakes which are still very distinctly felt in Scandinavia. The traces of these risings consist here, as in many other countries, chiefly of remains of marine animals left in places which are now elevated to a height of several hundred feet above the level of the sea, and also of ancient shore lines which are found dry at certain distances from the present margin of the sea. In the "*Magasin for Naturvidenskaberne* (2d Series, vol. i.), I

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* Vide Wernerian Memoirs, for Adamson's Observations on the Recent Sea Shells found on the shores of Loch-Lomond.

have described a shore line formed at an early period by the Gulf of Drontheim, at the foot of a sandy bank, near Steenkjor, and now situated about twenty feet above the Fjord. In the same journal I have also indicated the horizontal channellings which have been remarked in the prefecture of Nordlands, and in Finmark, not only in the loose soil, but also in the hard rock, at a height of from fifty to one hundred feet above the level of the sea. We must doubtless refer also to this class of facts, certain accumulatious of rolled blocks ranged in a parallel manner at the margin of the sea, which have been observed at Sandmoor by M. Schive, the inspector of lighthouses, who will soon publish a notice on the subject. As to the other class of facts in question, many of the phenomena have already been made known to geologists, by the travels of MM. de Buch, Hisinger, and Brongniart. But last year I made some new researches, which have led me to some general results. After having, during previous expeditions, examined many localities in the north (*Magazin for Natur*, l. c.), and in the south of Norway, where the shell gravel occurs which has been examined in other quarters by the authors I have just cited, and where also the clay has been found, containing marine shells, observed near Steenkjor by Von Buch, I devoted nearly the entire vacation of last summer to the investigation of these same deposits. My friend M. Boeck, Professor at the Veterinary School of Christiana (who is about to make himself known to geologists by a monograph on trilobites), and myself, surveyed more particularly the prefecture of Smaalehnene, where these deposits occur most frequently. I need not remind geologists of the very interesting discovery made by M. A. Brongniart, of the bases of Balani still adhering to the rock, at an elevation of nearly 200 feet above the level of the sea, near Uddevalla, in Sweden. The same fact presented itself to us at a place called Hellesaaen, about eight leagues distant from the coast, and elevated about 430 Paris feet above the level of the sea. We saw the shell gravel at several places where it had not been previously noticed; and there were always at least some of the shells, even the most fragile, in a state of conservation so perfect, that there cannot be a doubt of the gravel having been formed on the spots where it now reposes. As to the clay, it appeared to us pretty certain that all the great argillaceous deposits so widely spread over the S. E. of Norway, often having a thickness of upwards of 100 feet, and from which clay for bricks is obtained, belong, without exception, to the same formation, although it appears that in certain places they contain no fossils. We collected about fifty species of shells in the gravel and in the clay. M. Deshayes has examined nearly the whole collection, and has detected no species which do not occur in a recent state in the North Sea. (All the fossils of the shell gravel of which M. Hisinger has lately given the enumeration, are living species.) Besides the shells, we may also cite the skeleton of a whale which was discovered in 1682, near Frederikshald, in the clay of Fistedalen, and another found in the same formation in the valley of Stordalen. Some remains of fishes and echini enveloped in hardened clay, and brought from Romsdalen and Nordmor, seem to have been derived from the ordinary clay of which we speak. Finally, the marine plants in the peat of Oreland, already remarked by M. Fabricius, will complete the list of organic remains contained in these recent deposits. Much has been said of bones of whales which it is pretended have been found at considerable elevations in Nordland and Finmark, but hitherto they have not been seen by any naturalist, and we cannot yet range such assertions among our citations of facts, although they seem by no means improbable, especially since the discovery made by Ross in Lancaster Sound. Without dwelling more on these particulars, I shall now only mention the results to which I have been led by my researches on marine deposits, regarded as traces of the rising (*soulevement*) of the surface of Scandinavia. 1*st*, The clay in question (that is, the common clay of Norway, which is used for the manufacture of bricks), the shell gravel and the peat, at Zoffera, indicate, by the different level of the masses they form, several reiterated risings (*soulevemens*). 2*d*, The argillaceous deposits, more especially, occur at different elevations, and forming several terraces, some being at a greater height than the others. The maximum height of these terraces seems to be about 600 Paris feet. 3*d*, The shell gravel deposits being met with from North Sweden to Finmark, the upraised districts must have had a very considerable extent; there is certainly no reason for supposing that each shock must have acted on all that part of Scandinavia, but still a very remarkable equality in the distribution of the masses in question supports the belief that at least some of the soulevemens must have been almost general.* Besides the ancient shore-lines and the marine deposits of which I have now spoken, there is still a curious fact which is probably connected with the same Scandinavian risings (*soulevemens*). In many places, on the high mountains, the limits of vegetation seem to have descended. Roots of trees are found where shrubs hardly grow at present; forests of pines (Pinus sylvestris) terminate on the flanks of mountains by lines of dead trees, which, however, have remained in their upright position for several ages, &c. This fact has been observed not only in Sweden, but also in Norway, which does not seem to undergo the same gradual rising as the eastern part of Peninsula. As to this last movement of the surface of Scandinavia, I have hazarded the conjecture, that even this change of level, which, according to ordinary views, is altogether unique of its kind, is to be attributed to shocks that have taken place during earthquakes, but that these shocks are inconsiderable, and that the soulevement which results from them is only perceptible after their long-continued repetition.—*List of Fossils of the Norwegian Shell Gra-*

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* Even in Spitzbergen, I have remarked beds of clay analogous to those of Scandinavia of which we are now speaking, and elevated about twenty feet above the shore. In these beds I found the Buccinum carinatum, a shell peculiar to the Polar Sea.

eel :—Corbula nucleus, Lam.* Corbula pisiformis.** Mya truncata.** Lutraria Boysii.* Amphidesma, n. sp.* Saxicava rugosa, Lam.* Saxicava arctica, Desh.* Saxicava pholadis, Lam.* Tellina, n. sp.* Lucina radula.* Venus radiata, Brocchi.* Astarte, n. sp.* Astarte, n. sp.* Astarte, n. sp.* Cyprina Islandica.* Cardium edule, var.* Cardium echinatum.** Arca, n. sp.* Nucula rostrata, Lam.* Nucula, n. sp.* Nucula Margaritacea.** Mytilus umbilicatus, Pennant.* Mytilus edulis.* Pecten pseudamusium, Chemn.* Pecten islandicus.* Ostrea Margaritacea, Lam.* Ostrea ——. .* Anomia Ephippium, Lam.* Dentalium cutalis.** Dentalium dentalis.** Patella ——. .* Emarginula fissura.* Rinuda (Defr.) sp. n.* Bulla lignaria.* Natica clausa, Lyell.* Turritella terebra.* Turbo littoreus, Lam.* Trochus cinerarius.** Cerithium reticulatum.* Fusus corneus.* Fusus peruvianus.* Rostellaria pes pelicani.** Buccinum undatum.* Buccinum reticulatum.* Balanus sulcatus.* Balnus ——. .* Balanus ——.* Serpula ——. .* Serpula ——. .* Nullipora polymorpha.**

5. *M. de Collegno on the Soulevements of the Hills of Superga.*—At a recent meeting of the French Academy of Sciences, an Essay was presented by M. H. de Collegno on the geological constitution of the hills of Superga, near Turin. The following are the conclusions deduced by the author from his investigations, and which, while they confirm the observations made many years ago on this well known locality by M. A. Brongniart, at the same time indicate some interesting results obtained by M. de Callegno, connected with M. Elie de Beaumount's views on *Soulevements*. 1. The hills of Superga are composed of beds belonging to three different formations, viz. the upper chalk, the middle tertiary formation, and the upper tertiary formation. 2. The present *relief* of the surface of these hills results from three distinct movements, which have taken place each at the termination of one of these periods and at the commencement of another. 3. The relative age of these "soulevements," although it is not proved to us by phenomena so well marked as those pointed out by Beaumount in great chains of mountains, is nevertheless well determined by the discordant arrangement of the beds of the different formations. The three dislocations observable in the hills of Superga correspond to the soulevement of the three chains of mountains which divide or surround Italy, viz. the great systems of the Apennines, the Western Alps, and the Eastern Alps.

6. *On the Presence of Cobalt and other metals in the Upper Sandstones of the Tertiary Formations of Paris.*—At the meeting of the Academy of Sciences of the 29th February, M. Alexander Brongniart communicated a very interesting discovery recently made by the Duke of Luynes, who has detected the presence of cobalt in the proportion of at least one per cent. of manganese, and of traces of copper and arsenic, in the ferruginous sandstone of the abandoned quarry of Saint-Clair, near

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* Species determined by M. Deshayes at Paris.

** Species determined by M. Deslongchamps at Caen.

Orsay, and in that of the quarry of Seaux-les-Chartreux, near Palaiseau. In the "Geognosie du Bassin de Paris," published by Brongniart and Cuvier in 1822, mention is made of the occurrence of deposits of arenaceous limonite and scattered nodules of hydrate of iron, in the same position as the cobalt has now been found; and in 1835, M. Brongniart observed in the sand accompanying the millstones of the quarry of Tarteret a thin bed of red argillaceous sand, which, from its external characters, he suspected to contain manganese; and this idea was confirmed by the analysis made by M. Malagutti. In the deposits of marly gypsum near Paris, manganese has been found in concretions, and having a dendritic arrangement; and zinc has been detected in the arenaceous limestones which form the transition from the gypsum formation to the calcaire grossier. Besides iron, manganese, and zinc, no other metal has hitherto been discovered in the tertiary series; but the discovery now announced proves that cobalt, a metal formerly regarded as one of the most ancient, whose presence had not been detected even in the chalk or Jura formations, either in veins or disseminated portions, exists disseminated, and *in an original position*, in the upper sandstone of Paris, that is, in the middle tertiary formation. In the present case the cobalt is accompanied by the substances generally associated with it, copper and arsenic, but also by manganese, which has never been found combined with it except in one instance. The only other authentic example of manganesian cobalt, being one mentioned by the Duke of Luynes himself, as occurring at Rengensdorf, in Alsace, at which locality a compound is found having great analogy to that of Orsay, but in a very different geological position, viz. in a vein of quartz traversing clay-slate. M. Brongniart remarks, that, if we carry our researches further, and examine what are the metals which have been brought to the surface of the earth during or after the tertiary period by volcanic action, we find that in lavas, basalts, trachytes, &c. there occur disseminated, iron, manganese, titanium, and copper, but no cobalt; and in veins, lead, zinc, antimony, silver, gold, and tellurium, but still no cobalt. Among the products of volcanos at present in action we find frequently arsenic, selenium, copper, and iron; but the only indication of cobalt hitherto discovered, was in a salt of cobalt observed by Davy in one instance in Vesuvius. M. Brongniart terminated his communication to the Academy by stating his belief that geologists, by careful investigation, will succeed in detecting manganese, cobalt, and zinc in other localities of tertiary rocks, for these metals "must have been introduced into the tertiary strata of Paris by powerful and, consequently, general causes, and there is no example on the face of the globe of a phenomenon limited to one single point."

7. *On the Colours of Flowers.*—A curious essay on this subject, entitled "*Die farben der Blüthen,*" was published last year at Bonn, by Dr Macquart, from which we extract the following abstract of the results obtained:—1. All flower leaves are originally green in the bud. 2. *Chlorophyll* contains no nitrogen. 3. All the tints of flowers are produced by

two colouring matters. 4. These colouring matters are produced by the action of the living principle on *Chlorophyll.* 5. When water or its elements are removed from *Chlorophyll*, *Anthokyan* is formed. 6. *Anthokyan* is the colouring matter in blue, violet, and red flowers. 7. By the addition of water *Anthoxanthin* is formed from *Chlorophyll.* 8. *Anthoxanthin* is the colouring matter of yellow flowers. 9. Besides these two colouring matters we find in white, blue, red, and violet flowers, a flower resin, which may be regarded as the transition between *Chlorophyll* and *Anthokyan.* 10. There is also a slightly coloured extractive matter in white and yellow flowers, which is to be considered as the colourless sap of the cells. It is remarkable for its extreme sensibility in regard to alkalies, which colour it yellow. 11. The form of the cells has no influence on the production of a certain colour. 12. Orange-yellow flowers contain both colouring matters, *Anthoxanthin*, and *Anthokyan* which is reddened by acids. 13. Brown flowers contain *Chlorophyll*, and *Anthokyan* that is reddened by acids. 14. Flowers which contain both colouring matters produce *Anthokyan* in the epidermis and the upper layers of the cells, but *Anthoxanthin* in the interior of the cells. 15. *Anthokyan* is also the colouring matter of the other red leaf-like organs, but is in such cases covered by a colourless epidermis. 16. A black colouring matter does not exist in leaf-like organs; plants concentrate so much a blue, violet, or green tint that it seems to us a black. 17. The alteration of the colour of flowers must be observed with reference to the different periods of the life of the plants. 18. Yellow proceeds directly from green. 19. After the period of fructification, yellow passes frequently to the opposite range of colours. 20. All buds of red and blue flowers pass from green through white to red. 21. White is the transition-step to blue. 22. Blue flowers are red in bud, because they have not begun to respire. 23. Some blue flowers become red and others white after the period of flowering. 24. The blue colour subsequently acquired by many red flowers may be explained in two modes.

List of Patents granted in Scotland from December 18. 1835, *to March* 18. 1836.

1835,

Dec. 18. To John Houldsworth of Glasgow, in the county of Lanark, cotton-spinner, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "certain improvements applicable to drawing and slubbing frames used in the manufacture of cotton and other fibrous substances."

24. To Joseph Skinner of Fen Court in the city of London, civil-engineer, for an invention of "improvements in machinery for cutting wood for veneers, and other purposes."

To John Joseph Charles Sheridan of Walworth, in the county of Surrey, alchymist, for an invention of "an improvement in the manufacture of soap."

31. To William Symington of Bromley, in the county of Middlesex, cooper, for an invention of "certain improvements in the steam-engine, and in the machinery and apparatus for propelling vessels by steam, which improvements are wholly, or in part, also appli-

cable as and to motive machinery of other descriptions, whether actuated by steam, or by any other moving power."

1836,

Jan. 8. To Elijah Galloway of Westmoreland Place, City Road, in the county of Middlesex, engineer, for an invention of "certain improvements in steam-engines, which improvements are applicable to other purposes."

To James Bullough of Blackburn, in the county of Lancaster, mechanic, for an invention of "certain improvements in hand-looms and power-looms."

11. To John Malam of Kingston-upon-Hull, in the county of York, civil-engineer, for an invention of "certain improvements in gas metres, and in the apparatus for generating gas for illumination."

14. To Joseph Whitworth of Manchester, in the county palatine of Lancaster, engineer, for an invention of "certain improvements in machinery for spinning, twisting, and doubling cotton, flax, wool, and other fibrous substances."

15. To William Harter of Manchester, silk manufacturer, for an invention of "certain improvements in machinery for winding, cleaning, drawing, and doubling hard and soft silk, which improvements are also applicable to machinery for winding, cleaning, and doubling thread or yarn, manufactured from cotton or other fibrous materials."

To Thomas Jevons of Liverpool, in the county of Lancaster, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "certain improved machinery to be used in manufacturing bars or wrought iron into shoes for horses, and also into shapes for other purposes."

18. To Thomas Greig of Rose Bank in the parish of Bury, in the county of Lancaster, calico-printer, for an invention of "a mode of embossing and printing at one and the same time, by means of a cylinder or roller, on goods or fabrics made of or from cotton, silk, flax, hemp, and wool, or any one or more of those materials, or on paper."

19. To Andrew Smith of Prince's Street, Haymarket, in the county of Middlesex, engineer, for an invention of "a new standing rigging for ships and vessels, and a new method of fitting and using it."

20. To John Day of York Terrace, Peckham, in the county of Surrey, gentleman, for an invention of "an improved wheel for carriages of different descriptions."

Feb. 1. To Moses Poole of the patent office, in the county of Middlesex, gentleman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention of "improvements in Jacquard looms."

To John Cooper Douglas of Great Ormond Street, in the county of Middlesex, Esquire, for an invention of "certain improvements in making vinegar from various materials, and in making useful articles from the refuse of such materials, and also in apparatus for applying and conducting heat to liquids to be used in the manufacture of vinegar and other purposes."

3. To Lightly Simpson of Manchester, in the county of Lancaster, alchymist, for an invention of "certain improvements in the preparation of certain colours to be used for printing cotton and other fabrics."

3. To John George Bodmer of Bolton-le-Moors, in the county palatine of Lancaster, engineer, for an invention of "certain improvements in machinery for preparing, roving, and spinning cotton and wool."

4. To James Brown of Esk Mill, in the parish of Pennycuick, in the county of Edinburgh, North Britain, for an invention of "a certain improvement, or certain improvements, in the making manufacturing of paper."

Feb. 4. To John Hewitt of Kinezie, Cornwall, gentleman, for an invention of "a combination of certain materials or matters which being combined or mixed together, will form a valuable substance or compound, and may be used with or as a substitute for soap."

12. To James Kean of Johnston, in the county of Renfrew, in the kingdom of Scotland, machine maker and engineer, for an invention of "an improved throstle flyer, or a substitute for an ordinary flyer, employed in spinning cotton, flax, hemp, wool, silk, and other fibrous substances."

18. To Edmund Ashworth of Egerton, in the county of Lancaster, cotton spinner, and James Greenough of the same place, overlooker, for an invention of "certain improvements in the machinery used in preparing and spinning cotton, silk, wool, and other fibrous materials.

To Franz Moll of Grove Lane Terrace, Camberwell, in the county of Surrey, Esquire, for an invention of "improvements in preserving certain vegetable substances from decay."

To Julius Jeffreys of Osnaburgh Street, Regent's Park, in the county of Middlesex, Esquire, for an invention of "improvements in curing or relieving disorders of the lungs."

27. To William Boulnois junior, of Gower Street, in the county of Middlesex, Esquire, for an invention of "an improved combination or arrangement of springs for carriages."

To Robert Griffith of Birmingham, in the county of Warwick, machine maker, for an invention of "improvements in machinery for making rivetts, screw blanks, and bolts."

March 1. To William Wainwright Potts of Burslem, in the county of Stafford, china and earthenware manufacturer, for an invention of "an improved method or process of producing patterns in one or more colours, to be transferred to earthenware, porcelain, china, glass, and other similar substances."

To John Baillie of Great Suffolk Street, Southwark, in the county of Surrey, engineer, for an invention of "improvements in propelling of vessels and other floating bodies by means of steam or other power."

4. To Miles Berry of the office for patents, No. 66 Chancery Lane, in the county of Middlesex, civil engineer and mechanical draftsman, in consequence of a communication made to him by a certain foreigner residing abroad, for an invention "for a certain improvement or certain improvements in power looms for weaving."

7. To William Wilson of the city of Glasgow, in Scotland, manufacturer, for an invention of "a method of making chains of wire."

8. To Charles Schafhautl of Sheffield, in the county of York, gentleman, for an invention of "improved gear for obtaining a continuous rotary action."

To Charles Schafhautl of Sheffield, in the county of York, gentleman, for an invention of "an improved steam generator."

To John Barsham of Stepney Causeway, in the county of Middlesex, oxalic acid manufacturer, for an invention of "improvements in the manufacture of oxalic and salacetocella."

15. To Clinton Gray Gilroy of Argyle Street, New Road, St Pancras, in the county of Middlesex, engineer, for an invention of "certain improvements in machinery for weaving plain and figured fabrics."

18. To Francis Brewin of the Kent Road, in the county of Surrey, Esquire, for an invention of "a certain new and improved process of tanning."

To James Morison of Paisley, North Britain, manufacturer, for an invention of "improvements on the jacquard machine, and on what is called the ten box lay, and on the reading and stamping machines used in making shawls and other figured work."

INDEX.

Animals, on the foot-marks of, in rocks, 179.

Arctic expeditions, remarks on, 93.

Arago, M., his notice concerning the life and writings of Professor Brinkley, 161.—On the nature of the light of comets, 170.—Biographical memoir of Dr Thomas Young, 213.—Questions for solution relating to meteorology, hydrography, and the art of navigation; 393.

Arts, proceedings of the Society of, 201, 419.

Atmosphere, composition of the, 205.

Aurora Borealis observed at Edinburgh November 18. 1835, 205.

Baily, Francis, Esq. his account of the Rev. John Flamstead, 131.

Bell, Sir Charles, notice of the appointment of, to the Chair of Surgery in the University of Edinburgh, 204.

Bear, notice of the polar, 208.

Beaumount, M. L. Elie de, on the structure and origin of Mount Etna, 185, 376.

Beroe, on a species of, by Robert Paterson, Esq., 26.

Biot, M., his abstract of the memoirs of John Napier of Merchiston, with notes, 255.

Bridge, account of the great suspension, at Fribourg, 123.

Brinkley, Professor, notice concerning the life and writings of, by M. Arago, 161.

Bromhead, Sir Edward Ffrench, his remarks on the arrangement of the natural botanical families, 245.

Bronn, Dr, notice of a work entitled Lethæa Geognostica by, 211,

Calculi, on some minute, found in the urinary bladder of an ox, by Dr Davy, 327.

Castor, description and drawing of a new pivot, by John Robison, Esq., 130.

Caterpillars, effect of intense cold on, 207.

Cavities, on the infra-orbital, of deers and antilopes, by Dr Jacob, 74.

Chalk, on the, and flint of Yorkshire, by James Mitchell, Esq., 68.

Civiale, Dr, his statistical researches on calculous affections, 173.

Clark, Dr, notice of his treatise on pulmonary consumption and scrofulous diseases, 209.

Climate of Palestine, 424.

Collegno, M. de, on the soulevements of the hills of Superga, 428.

Colours of flowers, on the, 429.
Comatula, account of, 295.
Connell, Mr, on the chemical constitution of gadolinite, 300.
Comets, on the nature of the light of, by M. Arago, 170.
Cosiguina, eruptions of the volcano of the, by Col. Juan Galindo, 165.
Cuvier, Baron, his biographical memoir of M. de Lamarck, 1.

Davy, John, M. D., on a curious phenomenon observed in the Island of Cephalonia, and on the proximate cause of earthquakes in the Ionian Islands, 116. His notice of some minute calculi found in the urinary bladder of an ox, 327.
Don, David, Esq., notice of the appointment of, to the Botanical Chair in King's College, London, 204.
Dufrenoy, M., on the volcanic formations of the environs of Naples, 126.

Eclipse, annular, of the sun, on 15th May 1836, 188.
Ehrenberg, Professor, his researches on the infusoria, 42.
Etna, on the structure and origin of, by Elie de Beaumont, 185, 376.
Expeditions, on some circumstances connected with the original suggestion of the modern Arctic, by the Rev. W. Scoresby, 93.

Faraday, Michael, Esq., his reply to Dr J. Davy, 37.
Flamsteed, short account of the Reverend John, by Francis Baily, 131.
Flora, Northern, notice of a, soon to appear, by Dr Murray, 211.
Forbes, Dr, notice of a Manual of Select Medical Bibliography by, 210.
Forbes, Professor, receives the Keith Medal, 405.

Gairdner, Dr M., his meteorological observations made at Fort Vancouver, 67.—Climate of Fort Vancouver, 205.
Galbraith, Mr W., on the powers and use of Kater's altitude and azimuth circle, 241.
Galindo, Colonel Juan, on the eruptions of the volcano of the Cosiguina, 165.
Gadolinite, on the chemical constitution of, 300.
Geology, 206.
Graham, Dr, his list of new and rare plants, 189, 412.
Gray, John Edward, Esq., his remarks on the difficulty of distinguishing certain genera of testaceous mollusca by their shells alone, 79.

Guthrie, Alexander, Esq., his account of a single reflecting microscope, 326.

Hepburn, J. Stewart, Esq., on the cause of obstruction in water-pipes, &c., 100.
Hope, Dr, his address on delivering the Keith medal, 405.
Horner, Leonard, Esq., on the occurrence of the Megalichthys in a bed of Cannel Coal, 309.

Infusoria, Professor Ehrenberg's researches on the, 42.
Ionian Islands, on the proximate cause of earthquakes in the, by Dr John Davy, 116.

Jenyns, Rev. Leonard, notice of a work on the British Vertebrate Animals by, 210.
Johnston, John, Esq., notice of a systematic treatise on the theory of draining land, &c. by, 210.

Lamarck, M. de, biographical memoir of, by Baron Cuvier, 1.
Lemming, effect of cold on the fur of the Hudson's Bay, 207.

Meteorology, questions for solution relating to, hydrography, and the art of navigation, by M. Arago, 393.
Megalichthys, on the occurrence of the, in cannel coal, by Leonard Horner, Esq. 309.
Microscope, notice of a single reflecting, by Alexander Guthrie, Esq. 236.
Mitchell, Dr James, on the chalk and flint of Yorkshire, 68.
Molasse, on the age of the, of Switzerland, 207.
Mollusca, remarks on the difficulties of distinguishing certain testaceous, by their shells alone, by John Edward Gray, Esq., 79.

Observations, meteorological, made at Fort Vancouver, by Dr M. Gairdner, 67.

Paine, Mr R. T., on the phases of the annular eclipse of the sun, 188.
Palestine, climate of, 425.
Paris, on the presence of cobalt in the upper sandstones of the tertiary formations of, 428.
Patterson, Robert, Esq., on a species of beroe, 26.
Patents, list of, granted in Scotland, 202, 430.

Pendulum escapement, description of a new detached, 303.
Pigeon, notice of the passenger, 209.
Planet, on the new one whose existence has been suspected, 423.
Plants, notice of the spontaneous growth of, 209.
Poisson, Dulong, &c., their report respecting the statistical researches of Dr Civiale on calculous affections, made to the Academie des Sciences, 173.
Publications, New, 209.

Railway, remarks on the Dublin and Kingstown, 320.
Robison, John, Esq., description of a new pivot castor, 130.
Royal Society of Edinburgh, proceedings of, 414.

Selby, J. P., Esq., his remarks on the quadrupeds inhabiting the county of Sutherland, 156,—on the birds, 286.
Scandinavia, facts relating to the rising of, 425.
Scotland, on the change in the relative levels of the land and sea on the west coast of, 425.
Scoresby, Rev. W., on some circumstances connected with the original suggestion of the modern Arctic expeditions, 93.
Society of Arts for Scotland, proceedings of, 201, 419.
Springs, on the temperature of hot and thermal, by Professor Bischof, 329.
Stevenson, D., Esq., his remarks on the Dublin and Kingstown railway, 320.
Superga, on the soulevements of the hills of, 428.
Sun, phases of the annular eclipse of the, by Mr R. T. Paine, 188.
Sutherland, notice of the quadrupeds and birds inhabiting the county of, by P. J. Selby, Esq., 156, 288.

Tables, on the Hindoo astronomical, 22.
Thompson, John V., Esq., his memoir on the star-fish of the genus Comatula, 295.

Vancouver, Fort, its climate, 205.

Witherspoon, Alexander, his description of a new detached pendulum escapement, 303.
Wernerian Natural History Society, proceedings of the, 197, 418.
Whale, notice of the black, 208.

Young, Dr Thomas, biographical memoir of, by M. Arago, 213.

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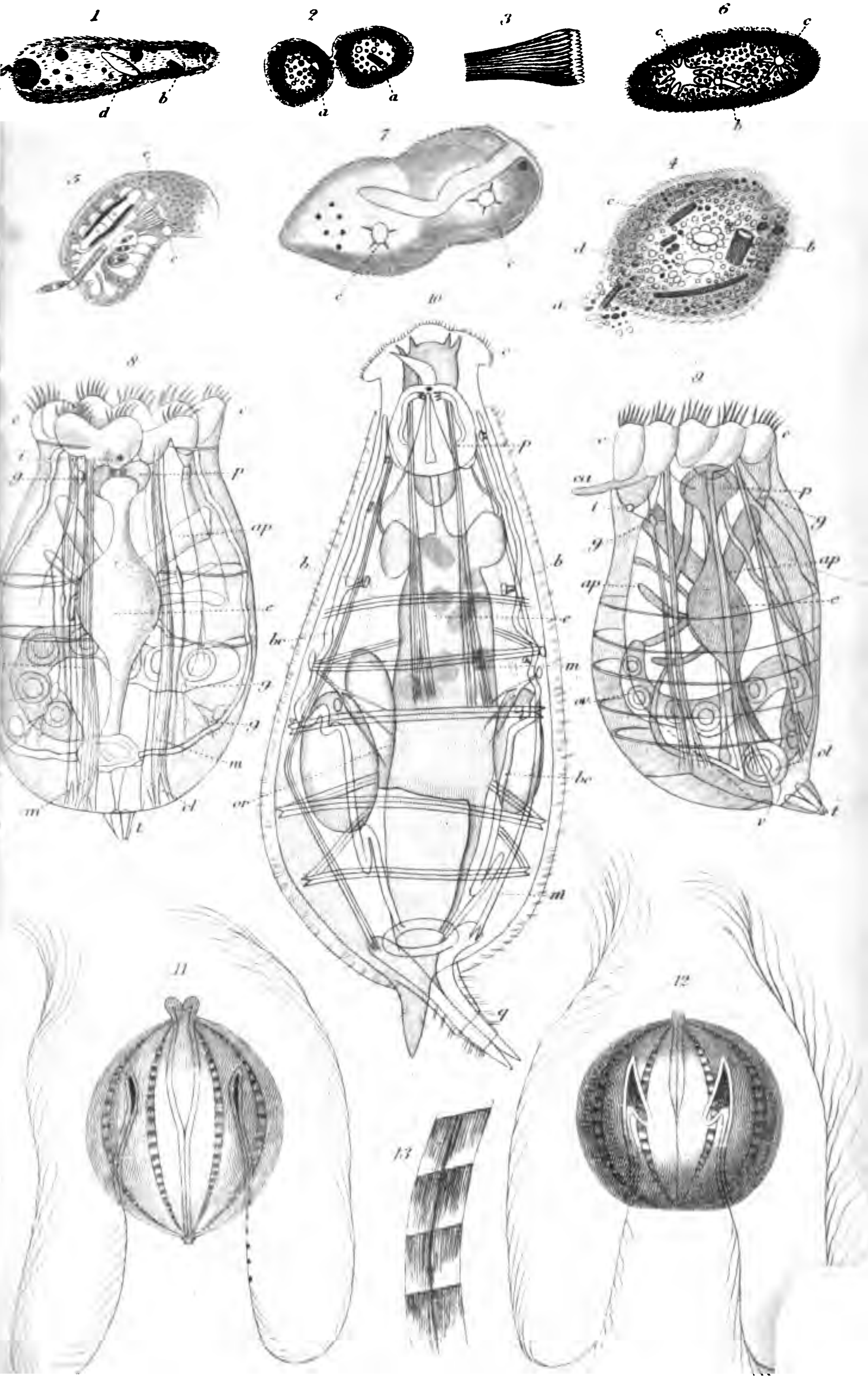
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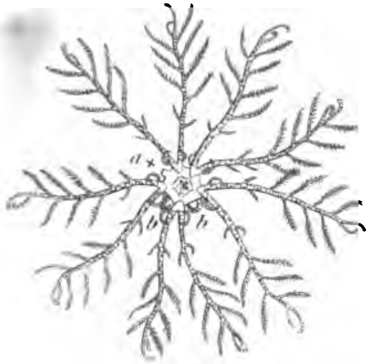

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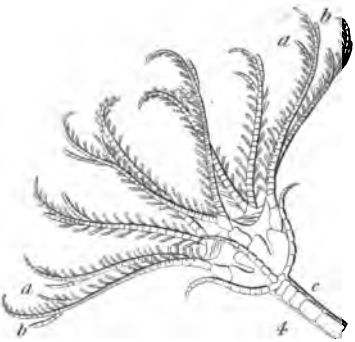

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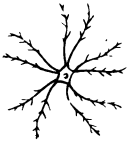

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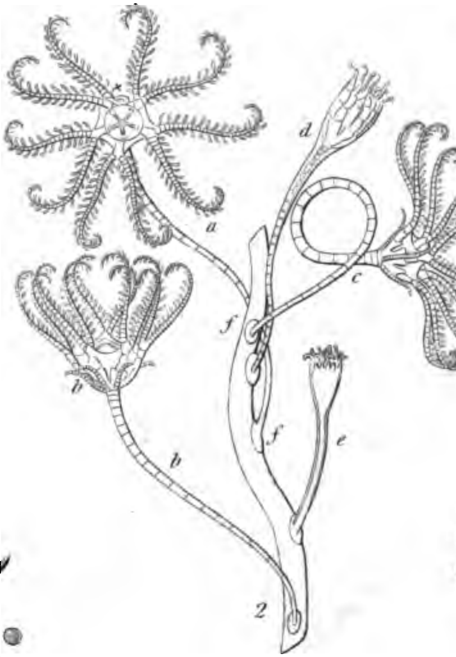

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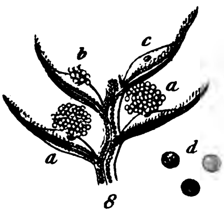

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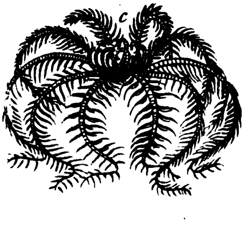

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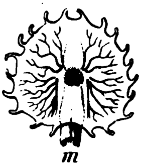

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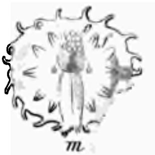
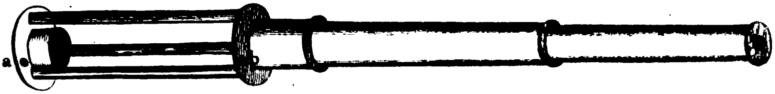


PLATE III. *Edinr. New Phil. Journ. Vol. XX. p. 326.*

Fig. 2.



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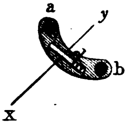

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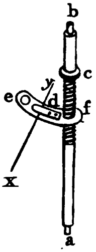

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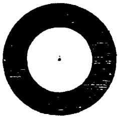

Mr. Guthrie's. Single Reflecting Microscope.

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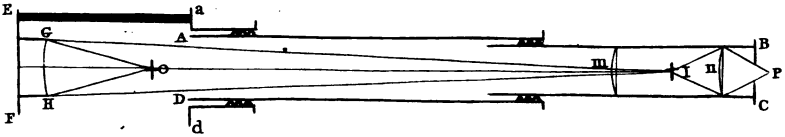
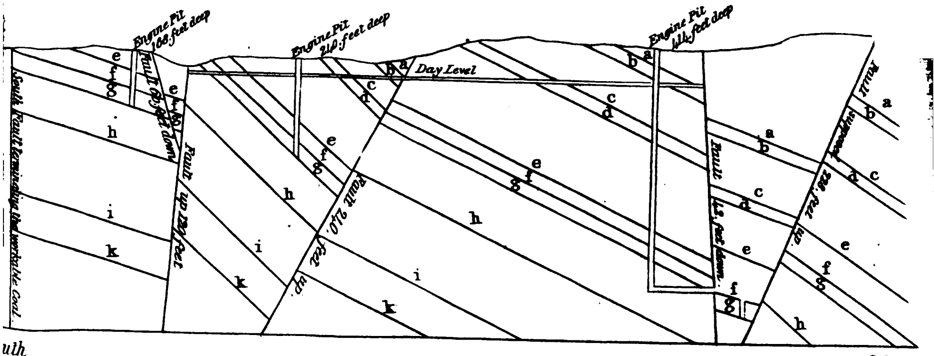
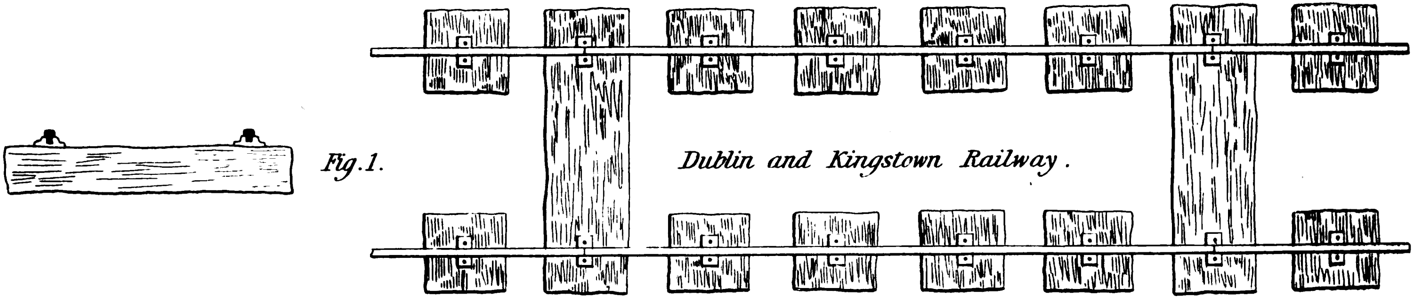


Transverse Section of the middle portion of the Coalfield of Hallbeath.



a to k. Coal Seams of various thickness. b. Cannel Coal in which the tooth of the Megalichthys was found.

PLATE IV. *Edin.r New Phil. Journ. Vol. XX.*



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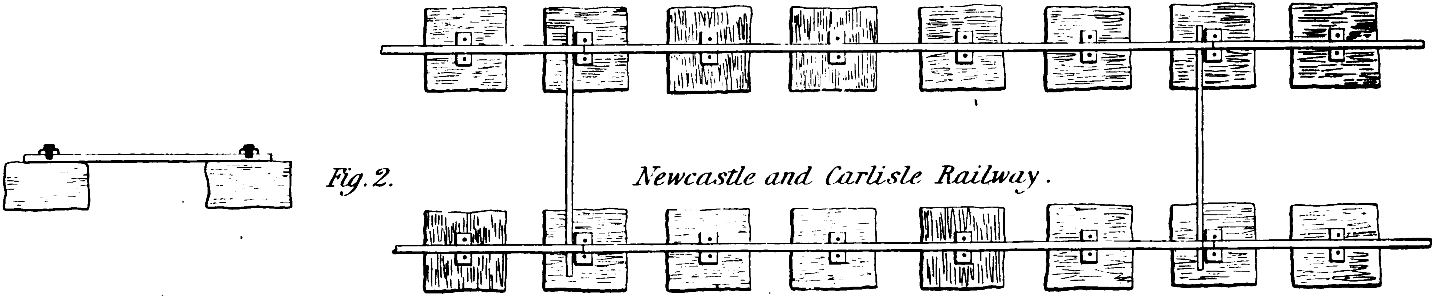

Fig. 2.

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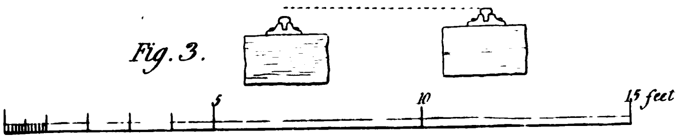

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